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Essays on Program Evaluation and Investment

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Abstract

This thesis presents three empirical essays related to liquidity, employment and investment. The first two essays evaluate the consequences of the largest public liquidity injection to the corporate sector in Spanish history. The first essay evaluates the consequences of this public program on the corporate behavior of firms and the second essay evaluates its impact on aggregate employment. The third essay characterizes the rise in importance of cross-listed securities and its implications for diversification across industries and countries.

Esta tesis presenta tres ensayos empíricos relacionados con liquidez, empleo e inversión. Los dos primeros ensayos analizan las consecuencias de la mayor inyección de liquidez pública al sector empresarial en la historia de España. El primer ensayo analiza las consecuencias de este programa público en el comportamiento de las empresas y el segundo ensayo analiza su impacto en el empleo agregado. El tercer ensayo describe el aumento de la importancia de las acciones “cross-listed” y sus implicaciones para la diversificación entre países e industrias.

Contents

1 Introduction

After the global financial crisis that was initiated in the last decade, Spain has not been able yet, to recover from a strong recession. The country has been experiencing a strong credit crunch and the Spanish government has undertaken many measures to overcome its dreadful consequences. The Spanish financial system experienced a strong restructuring process and many financial institutions were bailed out. Moreover, the Spanish government conducted the largest liquidity injection to the corporate sector in Spanish history. In 2012, over 30 billion euros were injected directly to firms through the FFPS (Fund for the Financing of Payments to Suppliers). Spain represents an ideal setting to study the consequences of a public liquidity injection when the banking sector is unable to satisfy the financing needs of the corporate sector.

In the first essay entitled “Liquidity provision: lessons from a natural experiment” (co-authored with Jose Maria Abad), we assess the economic impact of the FFPS plan on firms’ investment and financing decisions. More than 60,000 firms in Spain received more than 30 billion euros, a figure approximately equivalent to 3% of Spanish GDP. At that time, the Spanish economy was undergoing a strong credit crunch originated by the global financial crisis. In this setting, injecting liquidity to the banks was not increasing the supply of credit to firms and governmental direct cash transfers to firms seemed an interesting unorthodox stimulus policy. We assess the economic impact of the plan using two alternative estimation strategies. First, we use a differences-in-differences (DID) approach that exploits heterogeneity in the size of the liquidity received by firms. Second, we take advantage of the plan’s plausibly exogenous disbursement implementation and run a DID using as control group some firms that were paid a year later. Overall, we find a positive and significant reaction of corporate investment to the liquidity shock: on average firms use 4% of cash transfers for investment. This effect is stronger for firms with lower default risk and higher investment opportunities. Firms with higher default risk are more prone to repay financial debt. On average, firms use 8% of cash transfers to repay financial debt.

In the second essay entitled “How does easing liquidity constraints affect aggregate employment?” (co-authored with Jose Maria Abad and Rodolfo G. Campos), we again exploit the FFPS database to measure the impact of removing liquidity constraints on aggregate employment. We identify the effect on employment from the cross-sectional variation in the size of the liquidity shock to which the Spanish municipalities were exposed, and from how municipalities fared before and after the liquidity injection. Our findings indicate that the municipality where firms are located exhibits a stronger labor market response than the municipality where the liquidity injection originates. This result provides interesting insights on how firms' internal decisions can change the impact of government policies.

Finally, the third essay examines the role of cross-listed stocks in the diversification decisions of investors. "Do cross-listings substitute for international diversification?" (co-authored with José Manuel Campa and Rodolfo G. Campos), characterizes the rise in importance of cross-listed securities and its implications for diversification across industries and countries. We document a substantial increase in the number of stocks that are cross-listed and the relative market value of these types of securities. The rise in cross-listed stocks has allowed investors to diversify internationally from home by investing in these cross-listed stocks. The higher prevalence of cross-listing has been a driving force in the increase of the importance of industry effects relative to country effects. Thus, investment in a portfolio that includes cross-listed securities is an effective way to substitute for international diversification strategies.

2 Liquidity Provision: Lessons form a Natural Experiment

2.1 Introduction

In a recession, where should the government inject liquidity to foster investment and economic growth? To which extent does relaxing firms' financial constraints increase investment? Does this relation depend on firms' growth opportunities, debt maturity, or market conditions? The goal of this paper is to answer these questions with a clean causal identification strategy. We exploit a natural experiment that occurred in the biggest liquidity injection to the corporate sector in Spanish history. In particular, we evaluate the effectiveness of a liquidity injection program conducted by the Spanish government through the Fund for Financing Payments to Suppliers (FFPS). The program was introduced to expand economic activity and to overcome the strong recession that Spain was suffering. Spain was undergoing a strong credit crunch in 2012 and the government injected almost 30bn euros to alleviate firms' credit constraints and stimulate economic growth.

The crisis represented a negative shock to the supply of external finance for firms and this induced a decline of corporate investment (Duchin et al., 2010). In this setting, it is of great importance to determine the best way to channel liquidity into the economy to foster investment. Prior research has shown that reduced bank liquidity causes a reduction of credit supply to firms (Ivashina and Scharfstein (2010), Santos (2010) or Iyer et al. (2013)). In an attempt to alleviate banks' liquidity constraints, in 2009 the Spanish government created the Fund for Orderly Bank Restructuring (FROB), a program designed to bailout and reconstruct the Spanish financial system. After several bailouts, Spain received almost 39bn euros for bank recapitalization from the European Financial Stability Facility in 2012. Bank recapitalization and the effects of monetary policy have been widely studied (Ashcraft et al. (2011), or Diamond and Rajan (2011)), and precautionary hoarding and reluctance of banks to lend during a crisis have been evidenced. This was the case in Spain, where the credit crunch was especially severe and despite the large liquidity injection that Spanish banks received, access to finance was reported as the most pressing problem by almost 30% of the firms interviewed in a study by the European Central Bank (2014).

It is in this setting that the Spanish government introduced the FFPS program as an alternative and direct channel of liquidity to firms. Substantial literature has studied the effect of public spending on the real economy, and in particular on private investment, but results are far from being conclusive and have never been focused in recessionary periods (Barro (1981), Blanchard and Perotti (2002) or Kim and Nguyen (2014)). The case of Spain seems an ideal setting to study the potential impact of unorthodox stimulus policies during a recession when firms experience a serious shortage of credit supply.

We find that the plan was successful in stimulating investment. In particular, we find strong empirical evidence of a positive and significant relationship between liquidity and investment, which we claim is evidence of the effectiveness of the plan in alleviating the financial constrains caused by the credit crunch. Firms dedicated on average 4% of the cash received (1.1bn euros) to increase long-term investment.[1] This effect is stronger for firms with lower default risk and higher investment opportunities. Firms with higher default risk and lower investment opportunities are more prone to repay financial debt. On average, firms use 8% of cash transfers to repay financial debt.

The large unexpected liquidity shock conducted by the Spanish government (through the FFPS) in 2012 affected over 60,000 firms. In the five years prior to this shock, Territorial Administrations (both regional and local governments) had been accumulating arrears owed to suppliers. The volume of arrears was around 30bn euros, a figure as big as 3% of Spanish GDP. In February and March 2012, two laws were ratified by Parliament to set up the FFPS. The first appearance in the news of this measure was in mid-January 2012. All payments to suppliers were done in May/July 2012 through a cash transfer managed by the Spanish Official Credit Institute (ICO).

Our empirical strategy relies on the fact that the announcement and occurrence of the liquidity shock is confined to the short period between January and July 2012. Therefore, in December 2011 this shock was completely unexpected by any firm, and by December 2012 all firms had received the cash at least five months before. We use end of year financial data of the firms and have information from December 2007 up to December 2013. We use two methodologies, a DID approach that exploits heterogeneity in the size of the liquidity received by firms and a DID using as control group some firms that were paid a year later due to the plan's exogenous disbursement implementation. For both methodologies we use matching techniques to make the groups more comparable, although in the second methodology we show that firms are already very similar on observables.

Suppliers that worked for groups of municipalities (*mancomunidades*) that authorities had overlooked in the laws passed in 2012 received the payment of their

[1] Our estimates are relative to a control group, so these numbers must be interpreted as an increase in investment relative to our control group. This does not necessarily imply a net increase in investment if the control group reduces investment.

arrears a year later. These suppliers were paid in a second phase, together with other suppliers whose invoices were also not adequately processed in 2012. In total, more than 7,000 firms (representing arrears amounting to around 1bn euros) were paid in this second phase in 2013. We use this event as a refinement to contrast our main findings. We use the firms in phase 2 as our control group and study the impact of the liquidity shock of 2012 on the firms in phase 1. The advantage of using firms in phase 2 as our control group, is their resemblance in their characteristics to those in phase 1, and that they receive the shock a year later for exogenous reasons. By using these firms, we also control for potential endogeneity that arises from the specificities of firms that work for the public sector. In addition, we conduct our analysis with and without a matching strategy since the initial resemblance is not perfect and to avoid any biases originated from omitted variables.

Our study also contributes to the long-standing debate on the impact of firm's financial constraints on investment. There has traditionally been broad interest in the economic and financial literature on how financial frictions affect investment (Fazzari et al. (1988), Kaplan and Zingales (1997), Rauh (2006), Chen and Chen (2012) or Banerjee and Duflo (2014)). However, there is still no clear evidence on this relationship. For example, several papers have used data from the repatriation of foreign earnings under the American Jobs Creation Act to study the impact of changes in financial constraints on several corporate variables. Using the same data, while Blouin and Krull (2009) and Dharmapala et al. (2011a) find no effect on investment, Faulkender and Petersen (2012) find a positive and significant effect on corporate investment. In another influential paper, Rauh (2006) tries to identify the dependence of corporate investment on firm financial constraints. However, his work has been criticized due to his empirical specification: Rauh (2006) employs a regression discontinuity design, exploiting sharply nonlinear funding rules for defined benefit pension plans, and according to Bakke and Whited (2012), his results seem to arise from the use of a small fraction of the sample observations that have specific characteristics.

Our paper adds to the existing estimates of the effect of cash flows on investment through the use of a unique data set and a clean causal identification strategy. Correct identification of the causal effect of cash flows on investment is a challenge because both variables are co-determined in equilibrium. However, the natural experiment allows us to correctly disentangle the causality of the liquidity injection on investment. Spain is an ideal laboratory to test this puzzle because of the severe financial constraints that firms suffered during the Great Recession, as documented by Bentolila et al. (2013), Jimenez et al. (2014) or Bermejo et al. (2015). Jimenez et al. (2014) and Bentolila et al. (2013) use Spanish data on loan applications and grants from the Credit Register of the Banco de España (CIR) to disentangle the effects of credit supply and credit demand. Both papers show evidence in favor of a credit supply shock. The former finds that lower GDP growth caused a reduction in loan granting. They claim that this is especially relevant for a country like Spain, where most firms are bank dependent and bank substitution is difficult for constrained firms. The latter finds that the strong decrease of credit supply in Spain had negative effects on the real economy (they focus in the labor market effect). Interestingly, the FFPS program was precisely designed to alleviate firms' financial constraints, reduce corporate indebtedness, and foster economic growth. Therefore, it is a unique setting to study the impact of a liquidity shock on financially constrained firms.

Our data also allows us to determine whether the relation between investment and financial constraints depends on firms' characteristics. This has important policy implications, as government interventions could have different economic impact if they are channeled to different types of firms. We focus our analysis in the vast literature that analyzes the relationship between capital structure, growth opportunities and firm investment. Modigliani and Miller (1958) state that, under certain assumptions, the capital structure of the firm is irrelevant and that any firm with positive net present value investment opportunities would obtain funding to undertake them. Subsequently, many papers have stressed a negative relationship between leverage and investment when these assumptions do not hold. Myers (1977), for example, shows that with sufficiently high leverage, profitable projects can go unfunded because of the debt overhang created by prior debt financing. This effect has been commonly called the "underinvestment" problem of debt financing and implies that debt reduces the value of firms with growth opportunities. On the other hand, Jensen (1986)) or Stulz (1990) highlight the positive aspects of debt on investment, especially for low-growth firms, since debt can limit managerial discretion over free cash flows and avoid what has been called the "overinvestment" problem. In the same vein, in the literature on cash windfalls, Blanchard et al. (1994) show that firms do increase investment when they receive a cash windfall, by investing it in unattractive projects to avoid having outsiders with claims on this cash. It is clear that debt has a desirable moderating effect on investment when growth opportunities are low and "overinvestment" is plausible. However, debt and capital market imperfections can also have a negative effect on investment when interesting investment opportunities arise. Our research design allows us to better understand the impact of leverage and growth opportunities on the sensitivity of investment to liquidity.

Disentangling the effect of leverage and growth opportunities from the causal relation between liquidity and investment is challenging because liquidity, growth opportunities, leverage, and cash are jointly co-determined. By exploiting the unexpectedness of the liquidity shock received by the firms in 2012, and measuring growth opportunities and leverage prior to the shock, we observe their differential impact on the sensitivity of investment to liquidity. Moreover, we avoid additional sources of potential endogeneity that will be discussed later by exploiting the data from phase 2. In particular, we find that more leveraged firms use the liquidity received to repay debt, and that the firms that are more likely to invest are those with lower debt and greater growth opportunities. Banerjee and Duflo (2014) use variation in a targeted lending program to estimate whether firms are credit constrained. Contrary to our findings, they state that only constrained firms will use cash to expand production while unconstrained firms would use it as a substitute for other borrowing. However, they study firms in a unique industry (manufacturing), in a developing country (India), and in a setting with very different market conditions. We are aware that our results are specific for recessionary environments where credit supply shortage affects the corporate sector.

Our paper is also closely related to papers that study the differential effect of liquidity risk on short and long-term investment, and their impact on economic growth along the business cycle. Procyclicality theories argue that in a credit crunch, constrained firms are more concerned about the liquidity risk of long-term investments and reduce these investments in favor of more liquid short-term investments. Long-term investment has higher liquidity risk and thus firms with higher liquidity constraints are likely to reduce long-term investments. This effect is stronger in recessions, when liquidity is expected to be scarce. Aghion et al. (2012) show that R&D investment is countercyclical without credit constraints, but it becomes procyclical as firms face tighter credit constraints.

In our analysis, we focus on long-term investment. Long-term investment enhances productivity growth more than short-term investment (Aghion et al., 2010), it increases long-term economic growth and it is key to recover from a financial crisis (Garicano and Steinwender, 2014). We find that less indebted firms with higher growth opportunities exhibit a greater propensity to invest when they receive the liquidity injection. Our results are in line with those of Garicano and Steinwender (2014) who introduce a novel measure of credit shocks by observing the change from long-term investments to short-term investments of financially constrained firms. They find that firms prefer short-term investments that yield short-term cash flows since they want to mitigate the risk of being liquidated due to lack of access to cash. Our paper complements their paper: we empirically show a significant and different reaction of firms with heterogeneous probabilities of default when credit constraints are alleviated. We measure whether there is a significant reaction to a liquidity shock and study what firms actually do with the cash. As documented by Garicano and Steinwender (2014), reducing long-term investment impedes recovery from the financial crisis and reduces long-term economic growth. From a policy point of view, our results are key to determine which firms should the government target with a liquidity injection to increase long-term investment and foster economic growth.

The rest of the paper is organized as follows. In Section 2.2 we provide background information on the institutional setting in which the shock takes place. Section 2.3 describes the data used for the analysis and the construction of some relevant variables. The empirical strategy and summary statistics are shown in Section 2.4. Section 2.5 reports the results and Section 2.6 concludes.

2.2 Institutional Setting

The Spanish economy suffered a strong credit crunch originated by the financial crisis that burst in 2008 (Bentolila et al. (2013), Jimenez et al. (2014) and Bermejo et al. (2015)). The financial crisis had a substantive impact on the Spanish private sector, leading to higher unemployment and depressed domestic demand (Campos and Reggio, 2015a). The public sector was not left unscathed. Spain's public administrations, particularly at the sub-national level, experienced funding problems in the capital markets, just like local banks, and they also delayed payments to suppliers. The result was that, as of December 2011, the commercial debt accumulated by regional and local governments amounted almost to 30bn euros (almost 3% of GDP). This situation was creating a vicious circle: while mitigating the financial constraints of regional and local governments, it was augmenting the financial constraints that firms were already experiencing and hindering their recovery.

Therefore, aiming to address the liquidity problems faced by suppliers of regional and local governments, the Spanish government set up a new State-owned vehicle, the FFPS, through two Royal Decrees passed in 2012, February 24 and March 9.

On the asset side, the FFPS made payments directly to the suppliers of regional and local governments, subrogating itself in their position as claimants against these territorial administrations. As a result, commercial debt previously held by suppliers turned into financial debt in the hands of the FFPS. Interestingly, while participation by the 8,000 Spanish local authorities was mandatory, participation by the 17 regions was voluntary; and actually 3 of them (Basque Country, Galicia and Navarra) decided not to participate. Payments were made on three different dates: on May 28, 9.3bn euros were transferred directly to the suppliers of the 8,000 local governments; on June 25, 17.7bn euros were transferred directly to the suppliers of the 14 participating regions; and finally, on July 30, 0.3bn euros were transferred to the suppliers of local governments that had been left behind in the May payment. Overall, in just three bank transfers made in three different dates within a period of just two months, the ICO (the FFPS' paying agent) injected an amount of cash worth 27.3bn euros in the real economy.

Importantly, funding provided by the FFPS to the regional and local governments was guaranteed through the retention of their share of State tax receipts. Funding costs for regional and local governments equaled the Spanish Treasury's funding cost plus a maximum margin of 115 basis points to which a maximum mediation margin of 30 basis points was also added. These were quite favorable conditions compared to what regional and local governments could actually get by themselves in the capital markets. In order to avoid moral hazard, regional and local governments were required to submit a fiscal adjustment program to the Central Government. While regional and local governments complying with this requirement enjoyed funding with a maturity of 10 years with a 2-year interest-only grace period, funding provided to regional and local governments failing to meet this requirement was deducted from their share of State tax receipts over a 5-year period.

On the liability side, the FFPS gathered funds from a 30bn euros (maximum up to 35bn euros) syndicated loan granted by a pool of most of the Spanish banks. Given the State-owned nature of the FFPS, the syndicated loan was guaranteed by the State, making it attractive for participating banks. At the same time, however, all FFPS' liabilities became part of the central Government debt.

2.2.1 The second phase

In February 2013, another Royal Decree Law was ratified resulting in a new round (phase 2) of the FFPS. It was approved to pay the arrears to the suppliers of certain groups of municipalities (*mancomunidades*), a different sub-national entity that authorities had left behind in the laws passed in 2012, as well as claims which did not qualify in the 2012 payments due to different political reasons. Again, ICO transferred around 1bn euros to suppliers of regional and local governments.

The important fact for our analysis, is that the reason why some firms participated in this new phase was a matter of the slack of the laws passed in the first phase (they did not include *mancomunidades*) and political issues independent of the characteristics of the suppliers. In fact, not only were firms in both phases very similar in characteristics, but many of the suppliers in phase 2 also participated in phase 1. However, we conduct a matching procedure since we do find that firms that exclusively received funds in phase 2 are smaller from those of phase 1. Importantly, while payments by the FFPS extended over a 3-year window (2012-14), data exploited was restricted to the first 2 years (2012-13) since we need a window after the shock to capture the consequences of the liquidity injection.

Data from regions and municipalities is heterogeneous: this data shows significant heterogeneity in the payment behavior and financial strength of different regions and municipalities. This can lead to an endogenous relation of the suppliers that contract with different administrations, and thus must be taken care of. In our main analysis, we include geographical fixed effects to account for this heterogeneity, and in some cases we also control for the financial health of regions and municipalities.

2.3 Data and Sample

In this section we describe the data. All our data has annual frequency. Moreover, we also explain the construction of some variables used for our analysis.

We exploit a data set constructed by the Spanish Official Credit Institute (ICO). The ICO data set includes anonymous firm information from different phases of the FFPS and exhaustive firm-level data from the Iberian Balance Sheet Analysis System (SABI),[2]. Initially, in the first phase, the ICO data set includes anonymous information for firms accounting for 48.2% of all suppliers that benefited from the FFPS (64,879 out of 134,568) and almost 70% of the funds injected (19bn euros out of 27.3bn euros).[3] The data set includes information on the number and amount of unpaid bills that each firm has with each regional and local government, the amounts seized by the government due to unpaid taxes and social contributions and the dates in which the payments took place. The difference between the amount of unpaid bills and the seized amount equals the cash the firm effectively receives.

Interestingly, the ICO data set matches the information from the FFPS to SABI, a database with a coverage of more than 1.25 million firms in Spain. SABI data includes corporate accounting information, sector, number of employees, cash flow information and investment information. ICO's data set does not include information on 46,564 self-employed individuals (34.6% of suppliers and 1.5% of funds), nor on 23,125 firms (17.2% of suppliers and 29% of funds) that were not available in SABI.

Regarding the data from phase 2, a total of 1.14bn euros were injected to 5,070 firms. The ICO data set includes 1,848 firms, from which 1,201 are firms that already received funds in phase 1 (total amount of 259mn euros and average bill of 216,000 euros), and 647 are firms that only receive funds in phase 2 (total amount of roughly 80mn euros and average bill of 120,000 euros).

ICO obtained all the credit rating and probability of default data from a special financial strength indicator module available through SABI and provided by ModeFinance.[4] This data is an assessment of the creditworthiness of a company and it is based on a snapshot of the financial health of the company. These ratings are also provided on an annual frequency.

We obtain data on aggregate amounts of arrears and accounting information of counties and regions from the Spanish Ministry of Economy database.

Finally, we obtain from the Spanish Tax Agency the dates of each unpaid invoice. This information is useful to account for the unexpectedness of the liquidity shock.

In our analysis, we exclude financial firms, which means that a total of 156 firms are dropped from the FFPS sample. Moreover, we also drop firms that have no information on total assets.[5] We restrict our sample of treated and untreated observations to those

[2]SABI data is provided by INFORMA D&B in collaboration with Bureau Van Dijk

[3]Information on self-employed individuals and firms not covered in SABI (mainly financial firms and very small companies) is lost.

[4]ModeFinance is a data vendor that creates and develops a credit risk assessment methodology called MORE (Multi Objective Rating Evaluation)

[5]This implies that roughly 10.000 additional firms are dropped.

that have observations on all the matching covariates in the four years of our analysis.[6] Matching covariates are the same for all our regressions, so the sample is homogenous and results are comparable. By following this approach, we are aware that we are creating a survival bias, but we avoid an entry/exit bias whose consequences are far more unpredictable.

2.3.1 Our dependent variables: measuring corporate investment and short-term financial debt

We study the effect of the liquidity shock on investment and short-term financial debt. Each of these dependent variables is measured in a similar way.

We follow Asker et al. (2014) and measure corporate investment as the annual increase in gross fixed assets (gross property, plant and equipment) scaled by beginning-of-year total assets. As noted by Asker et al. (2014), by using this measure, we are capturing increase in assets due to both capital expenditures (CAPEX) and mergers and acquisitions (M&A). Our study is primarily focused on private firms which usually do not pay their acquisitions with stocks (due to their reduced size), so this measure seems to be the most appropriate to accurately measure corporate investment. As mentioned in Section 4.1, we focus on long term investment since it enhances productivity growth more than short-term investment (Aghion et al., 2010), it increases long-term economic growth and it is key to recover from a financial crisis (Garicano and Steinwender, 2014). Thus, investment for any firm i is:

$$Investment_{it} = (Fixed\,assets_{it} - Fixed\,assets_{it-1}) * 100/Total\ Assets_{it-1} \tag{2.1}$$

In a similar vein, in the case of short-term financial debt, we measure our variable of interest as the annual difference in short-term financial debt scaled by beginning-of-year total assets. Again, we multiply by 100 to scale coefficients, in order to interpret coefficients of regressors in percentage terms.

2.3.2 Measuring the liquidity shock by firm

All the invoices with different local and regional governments are aggregated at the firm level. Therefore, we have the total amount of arrears that each firm is paid. Information on seized amounts by the central government are also reported in our database. Seized amounts are due to debts that firms had with the central government. These seized amounts are deducted from the total amount of arrears that are owed to the firm. Our measure of the liquidity shock is the total amount of arrears minus the total amount

[6]Matching procedure is described in Section 2.4.1.

seized by the government. It is therefore a measure of the effective amount of euros transferred from ICO to the firm. We normalize the size of the liquidity shock by the firms' total assets in 2011. We normalize by the value in 2011 since it is the year prior to the realization of the shock and we don't want our measure of the liquidity shock to be affected by the shock itself (which happens in 2012). Thus, the liquidity shock for any firm i is:

$$Liquidity\,shock_i = (Total\;arrears_i - Seized\,amounts_i)/Total\;Assets\;in\;2011_i \quad (2.2)$$

2.3.3 Measuring default risk and investment opportunities

We disentangle the differential effect of default risk and investment opportunities on the relation between investment and the liquidity shock.

Faulkender and Petersen (2006) find that credit ratings exogenously affect a firm's access to financing. Our measures of credit ratings are firm-year probabilities of default. These probabilities are ranked from 0 to 100, with 100 being the highest probability of default.[7] Moreover, we also proxy credit ratings by computing adjusted Altman Z-scores and leverage, results are unchanged.[8] We divide the sample at the median to classify firms as having high or low default risk. We use values for 2011 to make sure they are exogenous to the liquidity shock. Several papers, such as Almeida et al. (2004), Campello et al. (2010) or Chang et al. (2014) have used credit ratings as proxies for financial constraints.

Traditionally, investment opportunities have been measured using either Tobin's q (ratio of the firm's market value to the book value of its assets) or sales growth. The majority of the firms in our sample are not traded on the stock exchange. In Spain, barely 200 firms are traded, so we use sales growth to proxy for investment opportunities. This measure has been profusely used in the literature, for example by Billett et al. (2007), Bloom et al. (2007), Michaely and Roberts (2012) or Asker et al. (2014). For each firm, we calculate the average sales growth for the two years previous to the shock (2010 and 2011). Subsequently, for each industry, we divide the sample at the median to classify firms as having high or low two-year sales growth.

2.4 Empirical Strategy and Summary Statistics

To analyze the consequences of the liquidity shock on investment and short-term financial debt, we use two differences-in-differences (DID) testing procedures. Matching variables, controls, rules to fix outliers and regression techniques are the

[7] Probabilities of default are provided by ModeFinance, as previously mentioned
[8] Altman Z-score is measured following Altman (1968).

same for both DID approaches and both variables of interest. In this way, results are made comparable.

We exploit the data from the FFPS that describes the repayment of arrears that Territorial Administrations had been accumulating. Given the specificities of firms that work for the public sector, it is difficult to decide on an appropriate control group to analyze the effect of this liquidity shock on investment. To avoid unobserved heterogeneity generated by these specificities, we restrict our analysis to firms that participated in the FFPS plan.[9] We exploit the heterogeneity in the size of the liquidity shock and use as treated firms those in the top tercile and as control group the firms in the lowest tercile. However, we find differences in firm characteristics among these two groups and therefore follow a matching procedure to control for these differences. Still, firms in the top tercile have a higher exposure to the public sector and we are concerned about the potential endogeneity that this can cause. To solve this, we take advantage of the plan's plausibly exogenous disbursement implementation and run another DID procedure using as control group some firms that were paid a year later. These firms are similar on all observables except in size, we correct this by matching firms in phase 2 with firms of similar size from phase 1.

2.4.1 Methodology 1: analyzing the heterogeneity in the size of the liquidity shock

Our first empirical strategy uses a DID methodology that exploits heterogeneity in the size of the liquidity injection and the time difference before and after the shock.

To construct the first difference, we separate firms (only firms with government arrears that participate in the first phase of the FFPS) in three quantiles according to the size of the liquidity shock they receive.[10] The first difference exploits heterogeneity in the size of the shock that the firms are exposed to. For that purpose, we drop the firms in the middle quantile in order to better gauge the effect of receiving a big liquidity shock versus the effect of receiving a smaller liquidity shock. The treated group will be formed by the firms exposed to the big liquidity shock (top quantile), and the control group will be composed by the group of firms that receive a smaller liquidity shock (bottom quantile).

There are significant differences in observables among the treated and control firms. Since both groups of firms appear to be different on several dimensions, they are likely to differ along unobservable dimensions too. Including control variables in a linear regression framework might not adequately control for unobservable heterogeneity between both quantiles (e.g., Irani and Oesch (2013)). Rosenbaum and

[9] In an extended version of this paper we also use firms randomly downloaded from SABI.
[10] Liquidity shock is scaled by assets in 2011, as noted in section 2.3.

Rubin (1983) propose propensity score matching as a method to reduce the bias originated by the estimation of treatment effects with observational non-random data sets. In order to achieve objective causal inference, we make use of matching techniques to try to approximate to randomized trial. To control for the potential endogeneity that non random data sets can cause, we use a matching approach to improve the resemblance of firms receiving a high and low liquidity shock.

To avoid endogeneity or spurious correlations it is important that our treated and control group are similar in all characteristics (observable and unobservable) that can affect investment (or the variables of interest analyzed in each case). As shown in Table 2.1, this is not the case. Table 2.1 shows means and standard deviations of a list of variables for the top and bottom quantiles (columns 1 and 2). Columns 3 and 4 in Table 2.1 report the means and standard deviations of the matched groups, both for the treated and control group. Table 2.2 develops t-tests to evaluate whether the differences of means for the unmatched and matched groups are significant. Column 1 reports the differences of the unmatched groups and column 2 reports the differences of the matched groups.

We adopt nearest-neighbor propensity score matching, each firm in the top tercile (treated firms) is matched to a unique firm from the bottom tercile (control firms). We choose a single match and allow for replacement (the same control firm can be used more than once as a match). We are more concerned on minimizing the bias at the cost of larger variance, since our sample is sufficiently large to be less concerned about variance (Abadie and Imbens (2002)). Moreover, to avoid biased coefficients, we set a caliper of 0.01.[11] This implies that some treated firms might not be matched if they do not have a control firm within the caliper chosen. That is the reason why we observe less firms after the matching is conducted in columns 3 and 4 of Table 2.1.

We match the treated and control group in size (measured by total assets), the growth rate of sales (proxy for growth opportunities), probabilities of default, corporate investment[12], profitability (measured by EBIT[13] to lagged assets) and industrial classification. We restrict the number of covariates since there exists a trade-off between the plausibility of the unconfoundedness assumption and common support (Black and Smith (2004)). According to Sianesi (2004), we must focus on covariates that simultaneously affect the treatment status (receiving a high liquidity shock) and the outcome variable (corporate investment). We have chosen these covariates since they have been proven to be determinants of firm investment decisions and are significantly different among firms in the top and bottom tercile groups. Once a match is formed, it is kept for the following years. We ensure that all potential matches have data on all the covariates for the whole sample.[14]

[11] A caliper sets a maximum distance of the propensity score for each treatment and its control.
[12] Measured as described in Section 2.3.1.
[13] Earnings before interest and tax.
[14] As reported in Section 2.3, if a firm does not have observations on all the matching covariates in the

We conduct the matching prior to the realization of the liquidity shock to make sure that our matching procedure is exogenous to any effects caused by the shock. We know that all variables included in the matching model must be unaffected by the treatment (the liquidity injection of 2012), and thus we carry out the matching by using the values of the covariates in 2010 and 2011. It is necessary that the treated and control groups follow parallel trends prior to the realization of the shock. We report the summary statistics and t-test results of 2011 in the appendix. The fact that our groups are comparable in 2010 and 2011 is evidence that our matching is robust and correctly specified.

Finally, the second difference is the time dimension, before and after the liquidity shock. Since we have yearly information on company financials by the end of the year, we define the period before the shock as 2010-2011, and the period after the shock as 2012-2013.

This allows us to estimate the differences-in-differences effect of a liquidity shock on investment: the difference between suppliers that receive a high versus a low liquidity shock (matched) and the difference between the period before and the period after the shock.

Our analysis does not only report results using the matched groups. We carry out baseline regressions to evaluate the data before the matching is done. These regressions are useful to compare raw results to those that arise from the matched groups and to better understand the effects of the techniques we apply. Our baseline equation is as follows:

$$y_{it} = POST_{(t\geq 2012)} + \sigma LiquidityShock_i \times POST_{(t\geq 2012)} + Firm_i + \beta X_{it} + \varepsilon_{it} \qquad (2.3)$$

where y_{it} is our variable of interest (corporate investment or short-term financial debt); $POST_{(t\geq 2012)}$ is a dummy variable (structural break) that takes value 1 for 2012 and 2013 and zero for years 2010 and 2011; $LiquidityShock_i$ is a continuous variable that captures the size of the liquidity shock;[15] $LiquidityShock_i \times POST_{(t\geq 2012)}$ is an interaction term; $Firm_i$ is a firm fixed effect and X_{it} is a vector of controls.

Controls are the same in all regressions and include total assets, capital structure (liabilities to equity), a bankruptcy dummy,[16] an interaction between the bankruptcy dummy and the capital structure variable, sales to lagged assets, return on assets, growth rate of return on assets, cash, probability of default, growth rate of sales, EBIT to lagged assets, short-term debt to long-term debt and a dummy for the firm's age.[17] All controls

four years of our analysis, the firm is dropped and not used to form the matched groups.

[15] It's construction has been described in Section 2.3.

[16] This dummy takes value 1 when the firm has negative equity.

[17] We construct three age bin classes corresponding to firm's created after 2005 (young), 1995-2005 (mid), and pre-1995 (old). This dummy is colinear with the firm fixed effect except when the firm changes age bin.

are measured prior to the shock. Lagged values are used for all years except for 2013, in which the value of 2011 is used.[18]

We compute an analogous regression in which instead of having a continuous measure of the liquidity shock, we use two groups (high and low) that will be used in our main DID methodology. We create a dummy that takes value 1 for the high liquidity group (top quantile or treated group) and zero for the bottom quantile.

Our main DID methodology is based in the following specification:

$$y_{it} = POST_{(t \geq 2012)} + \delta Grouphigh_{\{\ell_i \in L_H\}} \times POST_{(t \geq 2012)} + Firm_i + \beta X_{it} + \varepsilon_{it} \quad (2.4)$$

where $POST_{(year \geq 2012)}$ is analogous to that of equation (2.3); $Grouphigh_{\{\ell_i \in L_H\}}$ is an indicator of whether liquidity ℓ in firm i belongs to a group of high liquidity recipients (those in the top tercile); $Grouphigh_{\{\ell_i \in L_H\}} \times POST_{(t \geq 2012)}$ is an interaction term and our variable of interest; $Firm_i$ is a firm fixed effect and X_{it} is the vector of controls. The coefficient of interest is therefore δ, which indicates whether firms that receive a higher liquidity shock (top tercile and treated firms), invest more than firms that receive a lower liquidity shock (bottom tercile and control firms) after the realization of the shock.

Our DID methodology is adjusted to allow for an analysis within groups of default risk and growth opportunities. Groups are always created by separating firms in the sample in two quantiles above and below the median.[19] These groups are included in the regressions by interacting the dummies of the groups within our main specification. As an example, we report how the specification changes when we include default risk:[20]

$$\begin{aligned} y_{it} = & \sum_{j=Low,High} (\omega_j POST_{(t \geq 2012)} \times PDj_{\{PD_i \in PDj\}} \\ & + \gamma_j Grouphigh_{\{\ell_i \in L_H\}} \times POST_{(t \geq 2012)} \times PDj_{\{PD_i \in PDj\}}) \\ & + Firm_i + \beta X_{it} + \varepsilon_{it} \end{aligned} \quad (2.5)$$

where all variables are analogous to those in equation (2.4), except for $(PDj_{\{PD_i \in PD_j\}})$. $(PDlow_{\{PD_i \in PDlow\}})$ is equal to one for the firms with default risk below the median and zero otherwise, and $(PDhigh_{\{PD_i \in PDhigh\}})$ is equal to one for firms with probabilities of default above the median and zero otherwise.

[18] Alternative specifications have been used in which no controls are employed or in which controls are lagged values for all years including 2012. Results are unchanged. If controls from 2012 or 2013 are used, there is a risk that they are affected by the treatment. We are not interested in the mechanism in which the liquidity shock affects investment through other variables, so in our main methodology we avoid including control variables from 2012 or 2013. If post-treatment controls were added, then we would have to decompose the effect of the treatment to learn what part of the effect of the liquidity shock on investment goes directly through the shock, and what part affects investment through other control variables.

[19] Section 2.3 explains how groups of default risk and growth opportunities are created.

[20] The specification is analogous for growth opportunities.

The specification is as follows when we interact the groups of default risk and growth opportunities at the same time:

$$\begin{aligned} y_{it} = & \sum_{j=Low,High} \sum_{k=Low,High} (\rho_{jk} POST_{(t \geq 2012)} \times PDj_{\{PD_i \in PDj\}} \times Growthk_{\{Growth_i \in Growthk\}} \\ & + \lambda_{jk} Grouphigh_{\{\ell_i \in L_H\}} \times POST_{(t \geq 2012)} \times PDj_{\{PD_i \in PDj\}} \times Growthk_{\{Growth_i \in Growthk\}}) \\ & + Firm_i + \beta X_{it} + \varepsilon_{it} \end{aligned} \tag{2.6}$$

where all variables are analogous to those in equation (2.5), and $Growthk_{\{Growth_i \in Growthk\}}$ takes different values according to whether firm k belongs to the group with high or low growth opportunities.

As commented earlier, the size of the liquidity shock depends on the firm's exposure to local/regional governments. Thus, our methodology is exposed to potential endogeneity due to omitted variables if there are unobserved characteristics related to the size of the liquidity shock that affect investment (or the corresponding variable of interest) and are not captured by the matching procedure or the controls. A bigger amount of unpaid arrears might not only be correlated with size, financial constraints or industrial effects; but also with the "quality" of the governments that a firm works with or simply with the exposure of the firm to public institutions. We try to control for this potential endogeneity by including two sets of controls. To control for firms that work for "lower quality" municipalities (firms that work with municipalities with a lot of debt and that are worst payers), we construct the following measure per firm: $Quality = \sum_{i=1}^{n}(weight_i \times municipalitydebt_i)$ where i stands for municipality, n for the number of municipalities that a firm has unpaid invoices with, weight measures the percentage weight of unpaid amount in invoices with municipality i respect to all unpaid invoices, and municipality debt is measured as debt to income of municipality i. We also include as a control variable the number of municipalities that the firms works with. Still, it is impossible to assert that our identification strategy is completely clean of potential endogeneity. We overcome this weakness by taking advantage of a refinement described in Section 2.4.2.

2.4.2 Methodology 2: analizing the heterogeneity in the disbursement implementation

Our DID methodology described in Section 2.4.1 is exposed to potential endogeneity caused by the heterogeneity in the exposure of firms to local and regional authorities. The ideal experiment would compare firms that have the same exposure to the public sector. We try to overcome this fragility by exploiting the plan's plausibly exogenous disbursement implementation. The specific characteristics of this setting are described in Section 2.2.1.

Arrears owed to *mancomunidades* were paid a year later due to slack of the laws passed in phase 1. These *mancomunidades* are not concentrated in specific regions of Spain, in fact they are spread out and exist in all regional administrations of the country. Moreover, the industrial distribution of firms that work for *mancomunidades* is similar to those that appear in phase 1. Therefore, this second phase can be exploited as a refinement to measure whether there are significant differences on investment among the firms that participated in the first phase (received the liquidity shock in mid-2012), relative to those that participated in the second phase (received the liquidity shock in mid-2013). By exploiting this refinement, we control for potential endogeneity that arises from the specificities of firms that work for the public sector.

However, the drawbacks of this methodology are the amount of firms that exclusively receive the shock in phase 2 and the time spam after the shock. Since we use year end corporate data (December), firms only had several months to respond to the liquidity shock. They receive the money in May-July 2012, and we have financial data of the firms from December 2012. The information from December 2013 cannot be used in this analysis: the refinement would not be sufficiently clean since the firms in the second phase received the liquidity shock in August 2013.

We use firms in phase 1 as our treated firms and firms that only participate in phase 2 as our control group. We drop the firms that appear both in phase 1 and phase 2, since we want our control firms to participate exclusively in phase 2.[21] Firms in phase 1 and 2 are very similar on average, except for size. Since we have many more firms in the first phase, we match each firm in phase 2 with a peer firm from phase 1 to have common support, drop outliers, and improve the resemblance of the groups to better approximate to random trial.

Again, we follow a differences in differences testing procedure. However, in this case we measure the causal effect on investment for firms receiving the liquidity shock in 2012, and compare them to firms that do not receive the shock in 2012. Now, both groups have exposure to the public sector. To this end, we need both groups to be comparable. Our treated group are those firms in phase 1 that receive the cash in mid-2012. Our control group are those firms in phase 2 that receive the cash a year later. As reported in Table 2.3, both groups of firms appear to be very similar in all dimensions except for the size of the liquidity shock and total assets. Firms in phase 2 have less exposure to the public sector, but are bigger on average. As in our DID analysis, to control for the potential endogeneity that non random data sets can cause, we use a matching approach to create control-matched groups. We use a somewhat unusual procedure, since we have only 647 firms that can be used as controls, and more than 60.000 firms that are treated (firms that receive the shock in phase 1). We use all the firms in phase 2 (controls) and find a nearest neighbor for each control in the treated sample.

[21] The refinement is not as clean if our control firms receive cash also in phase 1. In this methodology we are particularly interested in achieving a clean identification.

To avoid endogeneity or spurious correlations, it is important that our two groups are similar in all characteristics (observable and unobservable) that can affect investment and debt. Table 2.3 shows means and standard deviations of a list of variables for the two phases (columns 1 and 2) prior to the shock (2011). We match both groups in the size of the liquidity shock, firm size (measured by total assets) and industry.

We follow the same criteria as in our main specification and adopt nearest-neighbor propensity score matching (Rosenbaum and Rubin (1983), Dehejia and Wahba (1999), Dehejia and Wahba (2002)). Each firm (for each of the two phases) that does not receive the liquidity shock (belongs to phase 2) is matched to a unique firm from phase 1 that receives the shock (treated firms). We choose a single match and allow for replacement (the same treated firm can be used more than once as a match). Once a match is formed, it is kept for the following year, we ensure that all potential matches have data on all the covariates for the whole sample (if not the firm is dropped from our sample). The means and standard deviations of the matched groups are listed in columns 3 and 4 of Table 2.3. We conduct the matching prior to the realization of the liquidity shock to make sure that our matching procedure is exogenous to any effects caused by the shock. We know that all variables included in the matching model must be unaffected by the treatment (liquidity shock), and thus we carry out the matching in 2011 (the year before the shock is realized).

Baseline equation:

$$y_{it} = \kappa POST_{(t=2012)} + \eta Phase1_{\{i \in Ph1\}} \times POST_{(t=2012)} + Firm_i + \beta X_{it} + \varepsilon_{it} \qquad (2.7)$$

where $Phase1_{\{i \in Ph1\}}$ is a dummy variable that takes value one for firms that participate in phase 1 and zero for firms that participate in phase 2 (this variable can also take a continuous value, being zero for firms in phase 2 and continuous in the amount of the shock received by the firm in phase 1); $POST_{(t=2012)}$ is a dummy variable (structural break) that takes value 1 for 2012 and zero in year 2011; $Phase1_{\{i \in Ph1\}} \times I_{(t=2012)}$ is an interaction term and X_{it} is a vector of controls. The coefficient of interest is η, which indicates the effect on investment of receiving the liquidity shock in 2012 versus not having received it yet.

The baseline equation interacted with default risk dummies is:

$$\begin{aligned} y_{it} = \sum_{j=Low,High} & (\tau_j POST_{(t=2012)} \times PDj_{\{PD_i \in PDj\}} \\ & + \phi_j Phase1_{\{i \in Ph1\}} \times POST_{(t=2012)} \times PDj_{\{PD_i \in PDj\}}) + Firm_i + \beta X_{it} + \varepsilon_{it} \quad (2.8) \end{aligned}$$

where the notation is analogous to equation (2.7) but now each term is interacted with the dummies of default risk. $(PDLow_{\{PD_i \in PDLow\}})$ is equal to one for the firms with default risk below the median and zero otherwise, and $(PDHigh_{\{PD_i \in PDHigh\}})$ is equal to one for firms with probabilities of default above the median and zero otherwise.

In equation (2.9), we show the specification used when we interact the groups of default risk and growth opportunities at the same time:

$$y_{it} = \sum_{j=Low,High} \sum_{k=Low,High} (\kappa_{jk} POST_{(t=2012)} \times PDj_{\{PD_i \in PDj\}} \times Growthk_{\{Growth_i \in Growth_k\}} + \mu_{jk} Phase1_{\{i \in Ph1\}} \times POST_{(t=2012)} \times PDj_{\{PD_i \in PDj\}} \times Growthk_{\{Growth_i \in Growth_k\}}) + Firm_i + \beta X_{it} + \varepsilon_{it} \quad (2.9)$$

where all variables are analogous to those in equation (2.8), and $Growthk_{\{Growth_i \in Growthk\}}$ takes different values according to whether firm k belongs to the group with high or low growth opportunities.

2.5 Results

Tables 2.5 and 2.6 use the specifications, matching techniques and controls described in Section 2.4.1. Table 2.5 reports the effects on investment and Table 2.6 reports the effects on short-term financial debt. Both tables follow the same exact structure. Column 1 reports results before the matching is applied, as described in equation (2.3) of Section 2.4.1. In column 1 the liquidity shock is continuous in the amount of the liquidity shock received by the firm. Columns 2 to 5 use the matched data. Column 2 is based in the specification described in equation (2.4). Columns 3 and 4 report results from equation (2.5), where groups of default risk and growth opportunities are separately and subsequently interacted with the variables of interest. Finally, column 5 is based in the specification in equation (2.6), where the groups of growth opportunities and default risk are interacted simultaneously with the variables of interest. Due to space availability and for clarity of exposition, only the coefficients of interest are reported.

Table 2.5 reports results on investment. Column 1 confirms that the relation between investment and the continuous amount of liquidity received by the firm is positive and significant. This result suggests that 3.5% of cash transfers were devoted to increase investment.[22] Since total cash transfers were around 27bn euros, this implies that this liquidity injection generated an increase of direct long-term investment of around 0.93bn euros. However, when the binary specification for the liquidity shock is used (column 2), the significance disappears. This is because all the within group heterogeneity is lost and both groups in aggregate are not significantly different. In column 3, the average effect on treated firms is divided into two groups according to their default risk. An important result arises: the sensitivity of investment to the liquidity shock is positive and significant for firms with low default risk. For every 100 euros received from the liquidity shock, firms belonging to the low

[22]Holding all other variables constant, we can assume this since we control for debt, cash or sales growth.

group of default risk invest on average 0.9 euros more than firms belonging to the high group.[23] In column 4 we recognize a positive response of investment to liquidity when firms have high growth opportunities, however the coefficient is not significant. In the last column we observe a positive and significant coefficient for firms with low probabilities of default and high growth opportunities. Among firms with high growth opportunities, for every 100 euros of cash received from the liquidity shock, firms that have low probabilities of default invest on average 1.4 euros more than firms with high probabilities of default.[24] Among firms with high default risk, for every 100 euros of cash received from the liquidity shock, firms that have high growth opportunities invest on average 0.6 euros more than firms with low growth opportunities.[25]

In Table 2.6 we observe a negative relation among the liquidity shock and the growth of short-term financial debt. Most firms that receive the higher liquidity shock seem to reduce short-term financial debt more than firms that receive the lower liquidity shock. This result is significant both for the continuous specification of liquidity (column 1), as well as for the binary specification (column 2). For every 100 euros of the liquidity shock received, firms dedicate on average 8.6 euros to reduce short-term financial debt. In other words, firms dedicate on average 8.6% of cash transfers to repay financial debt. For every 100 euros, firms in the high tercile of the liquidity shock dedicate 1.3 euros more to reduce short-term financial debt than firms in the low tercile. Results are much more significant for short-term debt than for investment. All firms seem to use the liquidity received to reduce debt. However, there is heterogeneity in the sensitivity to reduce debt that depends on firm characteristics that are captured by our groups of default risk and growth opportunities. Firms with higher risk of default and lower growth opportunities exhibit higher sensitivities to reduce short-term financial debt (columns 3 and 4 respectively). For every 100 euros, firms with higher default risk dedicate 1 euro more to reduce short-term financial debt than firms with lower default risk.[26] Similarly, for every 100 euros, firms with lower growth opportunities dedicate 0.6 euros more to reduce short-term debt than firms with higher growth opportunities. In fact, these results are confirmed in column 5 when these groups are interacted.

Tables 2.7 and 2.8 show results using the methodology described in Section 2.4.2. In this case, we take advantage of the plan's plausibly exogenous disbursement implementation.

[23] To obtain the differential impact on investment of belonging to low and high groups of default risk, we subtract the two coefficients of column 3: 0.545-(-0.353)$\approx$ 0.9. Both coefficients are significantly different. The average liquidity of both groups of default risk is not significantly different.

[24] We subtract the two coefficients of column 5 for high growth opportunities: 0.760-(-0.599)$\approx$ 1.4. Both coefficients are significantly different. The average liquidity of these two groups is not significantly different.

[25] We subtract the two coefficients of column 5 for high default risk: 0.760-(.157)$\approx$ 0.6. Both coefficients are significantly different. The average liquidity of both groups of low default risk is not significantly different.

[26] We subtract the two coefficients of column 3: -1.768-(-0.796)$\approx$ 1. Both coefficients are significantly different. The average liquidity of both groups is not significantly different.

In Table 2.7 we show the results for regressions in which the dependent variable is firm investment, and Table 2.8 measures the effects of the liquidity shock on short-term financial debt. Column 1 uses all firms in phase 1 and phase 2, no matching is carried out, therefore there are many more firms from phase 1. Column 1 shows results for equation (2.7), where phase 1 takes a continuous value. It takes value zero for firms in phase 2, and it is continuous in the amount of the liquidity shock received by the firm in phase 1. These results are very similar to those obtained in column 1 of Tables 2.5 and 2.6 respectively. There are small differences in the size of the coefficients due to the definition of outliers.[27] Column 2 is analogous to column 1 but the liquidity shock is a binary variable. Column 3 shows results for equation (2.8). Column 4 reports results for the high and low group of growth opportunities. Finally, column 5 interacts both groups as described in equation (2.9).

Table 2.7 corroborates results from Table 2.5. Firms with lower default risk and higher growth opportunities are more sensitive to invest once they receive the liquidity shock. However, although the sign and significance of the coefficients points in the same direction, the sizes are not comparable. In this section, the control group is made of firms that have not received any liquidity since they belong to phase 2, in Section 2.4.1 firms in the control group had received a lower liquidity shock relative to the treated group. It is therefore expected that the size of the coefficients in this section is larger than in the previous section for columns 2, 3, 4 and 5.

Table 2.8 shows that firms with higher default risk are more sensitive to reduce short-term financial debt. These results are similar to those obtained in Section 2.4.1. On average, firms dedicate 14% of cash transfers to reduce short-term financial debt.[28] In column 3 we corroborate that firms with higher default risk exhibit a higher sensitivity to reduce short-term financial debt once they receive the liquidity shock. Due to the reduced power problem mentioned, no significance is achieved for the regression with growth opportunities (column 4). However, the magnitude of the coefficients suggests that firms with lower growth opportunities seem to reduce more short-term debt.

Power is lost in these regressions relative to those in Section 2.4.1, since the number of firms used is much smaller. There is a trade-off between this methodology and the methodology presented in Section 2.4.1. In this case, a clean causal identification is pursued at the cost of using less firms and thus having less power.[29] In the analysis in Section 2.4.1, full exogeneity cannot be assured. However, results are robust, significance is strong and power is not an issue since sample size is big enough. By using both methodologies and obtaining similar results, we prove that our findings are robust and strong.

[27]In both methodologies, any firm that has missing values for a covariate used for the matching in the sample period is dropped. Since covariates and sample periods are different in both methodologies, results are not comparable.

[28]Again, as previously mentioned, there are small differences in the size of the coefficients due to the definition of outliers.

[29]Robust standard errors at the firm level are not shown since significance in this analysis is an issue.

2.6 Conclusion

In this paper we study the Spanish central government's decision to make a huge and direct liquidity injection to credit constrained firms during the banking crisis. The Spanish economy was undergoing a strong credit crunch in a severe recessionary environment. Using a unique data set and a clean causal identification strategy we find a positive and significant response of corporate investment to this unexpected governmental liquidity injection. This indicates that unorthodox stimulus policies can reactivate economic growth when the banking industry is not providing sufficient credit. Our estimates are the joint result of both the supply and demand of liquidity during the recession. The positive impact of a higher liquidity supply is moderated by the potential contemporaneous drop in liquidity demand as firms' investment opportunities vanish. Still, we find positive and significant reactions of investment to liquidity.

We also find that the impact of this policy is very different across firms. Our results show an heterogeneous reaction of firms to the liquidity shock. Firms with lower default risk and higher growth opportunities are more sensitive to increase investment, whereas firms with higher default risk, or that are highly leveraged, prefer to repay debt. From a policy perspective, given that the main objective of the governmental plan was to increase aggregate investment and to foster economic growth, our results give important insights on which are the appropriate firms to target with a public liquidity injection: firms with low default risk and high growth opportunities. It remains to be explored whether the liquidity injection also affected firms' labor policies and competitiveness.

Finally, we contribute to the debate on the sensitivity of investment to cash flows. The positive and significant response of corporate investment to the liquidity injection is evidence that firms were indeed financially constrained. We further quantify this sensitivity and find that firms invest on average 4% of the cash received. On the other hand, 13% of this liquidity is used to repay debt.

2.7 Tables

Table 2.1 *Quality of the match for the DID analysis in 2010*

	(1) GHLiq=1	(2) GHLiq=0	(3) GHLiq=1 on sup.	(4) GHLiq=0 (M)
Liquidity shock(contin)	0.13	0.00	0.12	0.00
	(0.23)	(0.00)	(0.18)	(0.00)
Total assets	7136.30	21096.52	8261.16	12299.10
	(60799.84)	(265236.15)	(66259.14)	(92927.01)
Employees	68.55	81.73	68.08	69.71
	(615.91)	(1016.87)	(648.18)	(600.99)
Probability of default	9.33	9.55	9.45	9.21
	(18.10)	(17.49)	(18.17)	(17.03)
ST debt to LT debt	6.27	8.97	6.64	9.59
	(37.66)	(94.29)	(40.68)	(103.97)
Investment	0.55	1.21	0.43	0.47
	(22.99)	(33.51)	(21.59)	(12.96)
Sales growth	0.15	0.02	0.03	0.01
	(5.69)	(1.87)	(0.94)	(0.90)
EBIT to lagged assets	4.17	3.02	3.03	3.02
	(18.98)	(19.09)	(14.73)	(11.35)
Altman Z-score	2.23	2.10	2.17	2.12
	(1.64)	(1.34)	(1.60)	(1.32)
Cash	519.62	1166.17	592.62	721.72
	(6220.09)	(24363.40)	(6815.17)	(9480.60)
Observations	10134	10134	7854	7854

This table reports summary statistics of firm-year observations in 2010 for the sample used for the DID analysis. Firms are classified as treated firms (column 1) if GHLiq = 1, which implies that these firms are classified in the top tercile regarding the size of the liquidity shock received, or as control firms (column 2) if GHliq = 0, which implies they belong to the bottom tercile. Columns 3 and 4 report summary statistics after the matching is realized. In column 3 we classify treated firms on support and in column 4 all control matched firms. Liquidity shock (contin) is the size of the liquidity size as described in Section 2.3.2, total assets and employees are measured in thousands, probabilities of default are described in Section 2.3.3, ST debt to LT debt is the ratio of short-term debt to long-term debt (includes debt issued and financial debt), investment and sales growth are calculated as described in Section 2.3.1 and Section **??** respectively, EBIT lo lagged assets is earnings before interest and taxes divided by lagged total assets, Altman Z-score is measured as in Altman (1968), cash is measured in thousands and includes cash and very liquid assets. Numbers reported are cross-sectional averages and standard errors in parentheses.

Table 2.2 *Ttest analysis: differences of means for unmatched and matched groups in 2010*

	(1) Mean differences	(2) Mean differences (Matched)
Liquidity shock(contin)	-0.127***	-0.120***
	(-56.02)	(-58.51)
Total assets	13960.2***	4037.9**
	(5.16)	(3.14)
Employees	13.18	1.635
	(1.10)	(0.16)
Probability of default	0.215	-0.238
	(0.86)	(-0.85)
ST debt to LT debt	2.698*	2.942
	(2.21)	(1.93)
Investment	0.665	0.0374
	(1.65)	(0.13)
Sales growth	-0.129*	-0.0250
	(-2.09)	(-1.71)
EBIT to lagged assets	-1.146***	-0.0100
	(-4.29)	(-0.05)
Altman Z-score	-0.128***	-0.0446
	(-6.11)	(-1.91)
Cash	646.6*	129.1
	(2.52)	(0.95)
Observations	20268	15708

This table reports t-test results of firm-year observations in 2010 for the sample used for the DID analysis. Column 1 of this table analyzes mean differences of the unmatched sample (columns 1 and 2 of Table 2.1). Column 2 analyzes mean differences of the matched sample (columns 3 and 4 of Table 2.1). Liquidity shock (contin) is the size of the liquidity size as described in Section 2.3.2, total assets and employees are measured in thousands, probabilities of default are described in Section 2.3.3, ST debt to LT debt is the ratio of short-term debt to long-term debt (includes debt issued and financial debt), investment and sales growth are calculated as described in Section 2.3.1 and Section **??** respectively, EBIT lo lagged assets is earnings before interest and taxes divided by lagged total assets, Altman Z-score is measured as in Altman (1968), cash is measured in thousands and includes cash and very liquid assets. T-statistics in parentheses. * $p< 0.05$, ** $p< 0.01$, *** $p< 0.001$

Table 2.3 *Quality of the match for phases 1 and 2 in 2011*

	(1) Phase2	(2) Phase1	(3) Phase2 on sup.	(4) Phase1 (M)
Liquidity shock(contin)	0.02	0.05	0.02	0.02
	(0.08)	(0.11)	(0.08)	(0.08)
Total assets	26475.91	7823.03	26475.91	27393.65
	(198715.89)	(89645.75)	(198715.89)	(226550.85)
Employees	57.97	47.81	57.97	104.09
	(432.56)	(588.45)	(432.56)	(720.86)
Probability of default	16.13	13.67	16.13	14.48
	(24.28)	(21.87)	(24.28)	(23.14)
ST debt to LT debt	11.40	8.56	11.40	7.34
	(91.00)	(243.61)	(91.00)	(45.09)
Investment	-0.37	0.05	-0.37	0.45
	(10.23)	(22.74)	(10.23)	(13.73)
Sales growth	0.04	0.21	0.04	0.17
	(0.73)	(29.84)	(0.73)	(1.90)
EBIT to lagged assets	-0.04	0.98	-0.04	0.92
	(18.66)	(29.32)	(18.66)	(20.60)
Altman Z-score	1.69	1.95	1.69	1.84
	(1.77)	(1.80)	(1.77)	(1.63)
Cash	344.71	500.55	344.71	365.85
	(1713.51)	(14890.41)	(1713.51)	(1326.64)
Observations	388	39007	388	388

This table reports summary statistics of firm-year observations in 2011 for the sample used for the refinement DID analysis. Firms are classified as control firms (column 1) if they receive the money in phase 2, or as treated firms (column 2) if they belong to phase 1. Columns 3 and 4 report summary statistics after the matching is realized. In column 3 we classify control firms on support and column 4 exhibits treated matched firms. Liquidity shock (contin) is the size of the liquidity size as described in Section 2.3.2, total assets and employees are measured in thousands, probabilities of default are described in Section 2.3.3, ST debt to LT debt is the ratio of short-term debt to long-term debt (includes debt issued and financial debt), investment and sales growth are calculated as described in Section 2.3.1 and Section **??** respectively, EBIT lo lagged assets is earnings before interest and taxes divided by lagged total assets, Altman Z-score is measured as in Altman (1968), cash is measured in thousands and includes cash and very liquid assets. Numbers reported are cross-sectional averages and standard errors in parentheses.

Table 2.4 *Ttest analysis: differences of means for unmatched and matched groups in 2011*

	(1) Mean differences	(2) Mean differences (Matched)
Liquidity shock(contin)	0.0274***	0.00186
	(6.37)	(0.31)
Total assets	-18652.9	917.7
	(-1.85)	(0.06)
Employees	-10.16	46.12
	(-0.45)	(1.04)
Probability of default	-2.456*	-1.646
	(-1.98)	(-0.97)
ST debt to LT debt	-2.837	-4.055
	(-0.47)	(-0.62)
Investment	0.419	0.828
	(0.79)	(0.95)
Sales growth	0.166	0.131
	(1.06)	(1.27)
EBIT to lagged assets	1.012	0.954
	(1.06)	(0.68)
Altman Z-score	0.256**	0.150
	(2.83)	(1.22)
Cash	155.8	21.14
	(1.31)	(0.19)
Observations	39395	776

This table reports t-test results of firm-year observations in 2011 for the sample used for the DID analysis. Column 1 of this table analyzes mean differences of the unmatched sample (columns 1 and 2 of Table 2.3). Column 2 analyzes mean differences of the matched sample (columns 3 and 4 of Table 2.3). Liquidity shock (contin) is the size of the liquidity size as described in Section 2.3.2, total assets and employees are measured in thousands, probabilities of default are described in Section 2.3.3, ST debt to LT debt is the ratio of short-term debt to long-term debt (includes debt issued and financial debt), investment and sales growth are calculated as described in Section 2.3.1 and Section **??** respectively, EBIT to lagged assets is earnings before interest and taxes divided by lagged total assets, Altman Z-score is measured as in Altman (1968), cash is measured in thousands and includes cash and very liquid assets. T-statistics in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

Table 2.5 *Effects on Investment (Methodology 1).*

VARIABLES	(1) Cont-Liq	(2) Match	(3) M-FC	(4) M-GO	(5) M-FC-GO
Post	-0.902***	-0.716***			
	(0.144)	(0.157)			
Post*Liquidity shock(cont)	3.455***				
	(0.772)				
Post*GroupHigh		0.075			
		(0.227)			
Post*GHLiq*PD-low			0.545*		
			(0.299)		
Post*GHLiq*PD-high			-0.353		
			(0.465)		
Post*GHLiq*Growth-low				-0.078	
				(0.268)	
Post*GHLiq*Growth-high				0.160	
				(0.468)	
Post*GHLiq*PD-low*Growth-low					0.157
					(0.393)
Post*GHLiq*PD-high*Growth-low					-0.241
					(0.340)
Post*GHLiq*PD-low*Growth-high					0.760**
					(0.384)
Post*GHLiq*PD-high*Growth-high					-0.599
					(1.099)
Observations	47,499	37,742	37,742	37,742	37,742
R-squared	0.007	0.013	0.013	0.013	0.014
Number of firms	14,510	11,385	11,385	11,385	11,385

This table presents estimates from panel regressions explaining yearly investment for the period 2010 to 2013. Post is a dummy variable that takes value one for years 2012 and 2013. Liquidity shock (cont) is the size of the liquidity shock received by the firm scaled by total assets in 2011, GroupHigh or GHLiq is a dummy variable that takes value 1 for the firms in the highest tercile of the liquidity shock and PD-low (PD-high) and Growth-low (Growth-high) are dummy variables that take value one for the firms in the bottom (top) half of probabilities of default and growth opportunities, respectively. Other controls include total assets, capital structure (liabilities to equity), a bankruptcy dummy (this dummy takes value 1 when the firm has negative equity), an interaction between the bankruptcy dummy and the capital structure variable, sales to lagged assets, return on assets, growth rate of return on assets, cash, probability of default, growth rate of sales, EBIT to lagged assets, short-term debt to long-term debt and a dummy for the firm's age. All controls are measured prior to the shock, so lagged values are used for all years except for 2013, in which the value of 2011 is used. All regressions include firm fixed effects. Within R-squared is reported. Robust standard errors clustered at the matched-firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5% or 10% level, respectively.

Table 2.6 *Effects on Short-Term Financial Debt (Methodology 1).*

VARIABLES	(1) Cont-Liq	(2) Match	(3) M-FC	(4) M-GO	(5) M-FC-GO
Post	-0.985***	-0.812***			
	(0.131)	(0.156)			
Post*Liquidity shock(cont)	-8.608***				
	(1.635)				
Post*GroupHigh		-1.303***			
		(0.252)			
Post*GHLiq*PD-low			-0.796***		
			(0.256)		
Post*GHLiq*PD-high			-1.768***		
			(0.414)		
Post*GHLiq*Growth-low				-1.613***	
				(0.346)	
Post*GHLiq*Growth-high				-1.035***	
				(0.365)	
Post*GHLiq*PD-low*Growth-low					-0.585
					(0.358)
Post*GHLiq*PD-high*Growth-low					-2.290***
					(0.514)
Post*GHLiq*PD-low*Growth-high					-0.969***
					(0.360)
Post*GHLiq*PD-high*Growth-high					-1.134*
					(0.682)
Observations	44,749	35,577	35,577	35,577	35,577
R-squared	0.023	0.022	0.023	0.022	0.024
Number of firms	13,838	10,859	10,859	10,859	10,859

This table presents estimates from panel regressions explaining yearly short-term debt for the period 2010 to 2013. Post is a dummy variable that takes value one for years 2012 and 2013. Liquidity shock (cont) is the size of the liquidity shock received by the firm scaled by total assets in 2011, GroupHigh or GHLiq is a dummy variable that takes value 1 for the firms in the highest tercile of the liquidity shock and PD-low (PD-high) and Growth-low (Growth-high) are dummy variables that take value one for the firms in the bottom (top) half of probabilities of default and growth opportunities, respectively. Other controls include total assets, capital structure (liabilities to equity), a bankruptcy dummy (this dummy takes value 1 when the firm has negative equity), an interaction between the bankruptcy dummy and the capital structure variable, sales to lagged assets, return on assets, growth rate of return on assets, cash, probability of default, growth rate of sales, EBIT to lagged assets, short-term debt to long-term debt and a dummy for the firm's age. All controls are measured prior to the shock, so lagged values are used for all years except for 2013, in which the value of 2011 is used. All regressions include firm fixed effects. Within R-squared is reported. Robust standard errors clustered at the matched-firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5% or 10% level, respectively.

Table 2.7 *Effects on Investment (Methodology 2).*

VARIABLES	(1) Cont-Liq	(2) Match	(3) M-PD	(4) M-GO	(5) M-PD-GO
Post	-0.665***	-0.873			
	(0.105)	(1.150)			
Post*Phase1-Liquidity(cont)	3.229***				
	(1.128)				
Post*Phase1		1.538			
		(1.570)			
Post*Phase1*PD-low			4.689*		
			(2.438)		
Post*Phase1*PD-high			-0.799		
			(2.052)		
Post*Phase1*Growth-low				-1.289	
				(2.291)	
Post*Phase1*Growth-high				4.135*	
				(2.160)	
Post*Phase1*PD-low*Growth-low					-1.660
					(4.163)
Post*Phase1*PD-high*Growth-low					-1.062
					(2.752)
Post*Phase1*PD-low*Growth-high					7.876***
					(3.028)
Post*Phase1*PD-high*Growth-high					-0.695
					(3.171)
Observations	44,580	879	879	879	879
R-squared	0.008	0.067	0.078	0.076	0.089
Number of firms	25,497	502	502	502	502

This table presents estimates from panel regressions explaining yearly investment for the period 2011 to 2012. Post is a dummy variable that takes value one for 2012. $Phase1 - Liquidity(cont)$ is the size of the liquidity shock received by the firm scaled by total assets in 2011, $Phase1$ is a dummy variable that takes value 1 for the firms that participate in phase 1 and zero for firms that participate in phase 2, and PD-low (PD-high) and Growth-low (Growth-high) are dummy variables that take value one for the firms in the bottom (top) half of probabilities of default and growth opportunities, respectively. Other controls include total assets, capital structure (liabilities to equity), a bankruptcy dummy (this dummy takes value 1 when the firm has negative equity), an interaction between the bankruptcy dummy and the capital structure variable, sales to lagged assets, return on assets, growth rate of return on assets, cash, probability of default, growth rate of sales, EBIT to lagged assets, short-term debt to long-term debt and a dummy for the firm's age. All controls are measured prior to the shock, so lagged values of the variables are used for all years. All regressions include firm fixed effects. Within R-squared is reported. Robust standard errors clustered at the matched-firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5% or 10% level, respectively.

Table 2.8 *Effects on Short-Term Financial Debt (Methodology 2).*

VARIABLES	(1) Cont-Liq	(2) Match	(3) M-PD	(4) M-GO	(5) M-PD-GO
Post	-0.998***	1.226			
	(0.145)	(1.301)			
Post*Phase1-Liquidity(cont)	-14.184***				
	(1.593)				
Post*Phase1		-2.208			
		(1.764)			
Post*Phase1*PD-low			0.283		
			(2.772)		
Post*Phase1*PD-high			-3.989*		
			(2.304)		
Post*Phase1*Growth-low				-3.250	
				(2.596)	
Post*Phase1*Growth-high				-1.241	
				(2.431)	
Post*Phase1*PD-low*Growth-low					-1.231
					(4.827)
Post*Phase1*PD-high*Growth-low					-4.033
					(3.105)
Post*Phase1*PD-low*Growth-high					1.012
					(3.434)
Post*Phase1*PD-high*Growth-high					-3.925
					(3.594)
Observations	42,484	840	840	840	840
R-squared	0.060	0.476	0.479	0.477	0.479
Number of firms	24,171	476	476	476	476

This table presents estimates from panel regressions explaining yearly short-term financial debt for the period 2011 to 2012. Post is a dummy variable that takes value one for 2012. Phase1-Liquidity (cont) is the size of the liquidity shock received by the firm scaled by total assets in 2011, Phase1 is a dummy variable that takes value 1 for the firms that participate in phase 1 and zero for firms that participate in phase 2, and PD-low (PD-high) and Growth-low (Growth-high) are dummy variables that take value one for the firms in the bottom (top) half of probabilities of default and growth opportunities, respectively. Other controls include total assets, capital structure (liabilities to equity), a bankruptcy dummy (this dummy takes value 1 when the firm has negative equity), an interaction between the bankruptcy dummy and the capital structure variable, sales to lagged assets, return on assets, growth rate of return on assets, cash, probability of default, growth rate of sales, EBIT to lagged assets, short-term debt to long-term debt and a dummy for the firm's age. All controls are measured prior to the shock, so lagged values of the variables are used for all years. All regressions include firm fixed effects. Within R-squared is reported. Robust standard errors clustered at the matched-firm level and shown in parentheses. ***, ** or * indicates that the coefficient is significant at the 1%, 5% or 10% level, respectively.

2.8 Appendix

2.8.1 Alternative methodology: flexible time model

An extended version of this paper makes use of a flexible model with year fixed effects instead of PRE and POST dummies (Mora and Reggio (2013)). This is useful to compare whether the two years before and after the shock are significantly different among each other or can be grouped together. We use 2010 as our baseline year and omit it.

2.8.2 Random download

We base the download on the CIFs (firm fiscal identification code) of the firms. CIFs are constructed by 9 characters, first character is a letter that indicates the legal type of the firm, the following two numbers indicate the region of the headquarters of the firm, the next five numbers depend on the time when the firm was registered in the Spanish Official Registry, and the last number is a control digit. To construct the random sample, we follow the subsequent steps:

1. Download all the CIFs that exist in SABI.
2. Eliminate all the CIFs of firms that participate in the FFPP.
3. Segment the remaining sample according to the legal types of firms.
4. For each legal type, keep all firms whose penultimate number is a one.
5. Calculate the percentage of the different legal types of firms that appear in the FFPS database.
6. Randomly download from each legal type of firm group as many firms as needed in order to have similar percentages as those observed for the FFPS database.
7. For some specific types, there are not enough firms of a certain legal type in my random groups. Our solution is to randomly download other firms whose penultimate number is not a one.

2.8.3 Analysis of the quality of the DID match for 2011

Table 2.9 *Quality of the match for the DID analysis in 2011*

	(1) GHLiq=1	(2) GHLiq=0	(3) GHLiq=1 on sup.	(4) GHLiq=0 (M)
Liquidity shock(contin)	0.13	0.00	0.12	0.00
	(0.23)	(0.00)	(0.18)	(0.00)
Total assets	7253.02	21263.17	8427.23	12368.68
	(62463.07)	(270868.76)	(68978.29)	(95951.87)
Employees	66.07	82.77	66.25	69.51
	(595.45)	(1035.55)	(632.51)	(584.94)
Probability of default	11.97	11.29	11.59	11.46
	(20.39)	(19.26)	(19.96)	(19.59)
ST debt to LT debt	5.99	8.76	6.15	9.35
	(38.15)	(107.28)	(39.49)	(119.36)
Investment	-0.60	0.11	-0.47	-0.35
	(9.91)	(20.55)	(9.90)	(11.35)
Sales growth	0.04	0.40	-0.01	0.01
	(1.28)	(29.13)	(0.42)	(0.58)
EBIT to lagged assets	1.33	1.60	1.31	1.42
	(16.60)	(14.23)	(14.78)	(11.37)
Altman Z-score	2.05	2.02	2.03	2.02
	(1.75)	(1.34)	(1.69)	(1.34)
Cash	466.90	1135.99	535.72	686.30
	(6593.61)	(29271.79)	(7380.04)	(8752.10)
Observations	10134	10134	7854	7854

This table reports summary statistics of firm-year observations in 2011 for the sample used for the DID analysis. Matching is done in 2010 as described in Section 2.4.1. Firms are classified as treated firms (column 1) if GHLiq = 1, which implies that these firms are classified in the top tercile regarding the size of the liquidity shock received, or as control firms (column 2) if GHliq = 0, which implies they belong to the bottom tercile. Columns 3 and 4 report summary statistics after the matching is realized. In column 3 we classify treated firms on support and in column 4 all control matched firms. Liquidity shock (contin) is the size of the liquidity size as described in Section 2.3.2, total assets and employees are measured in thousands, probabilities of default are described in Section 2.3.3, ST debt to LT debt is the ratio of short-term debt to long-term debt (includes debt issued and financial debt), investment and sales growth are calculated as described in Section 2.3.1 and Section **??** respectively, EBIT lo lagged assets is earnings before interest and taxes divided by lagged total assets, Altman Z-score is measured as in Altman (1968), cash is measured in thousands and includes cash and very liquid assets. Numbers reported are cross-sectional averages and standard errors in parentheses.

Table 2.10 *Ttest for the DID analysis in 2011*

	(1)	(2)
	Mean differences	Mean differences (Matched)
Liquidity shock(contin)	-0.127***	-0.120***
	(-56.02)	(-58.51)
Total assets	14010.2***	3941.4**
	(5.07)	(2.96)
Employees	16.70	3.256
	(1.39)	(0.33)
Probability of default	-0.679*	-0.128
	(-2.44)	(-0.40)
ST debt to LT debt	2.777*	3.206
	(2.01)	(1.86)
Investment	0.717**	0.123
	(3.16)	(0.72)
Sales growth	0.364	0.0247**
	(1.25)	(3.05)
EBIT to lagged assets	0.270	0.110
	(1.24)	(0.52)
Altman Z-score	-0.0344	-0.00704
	(-1.57)	(-0.29)
Cash	669.1*	150.6
	(2.18)	(1.13)
Observations	20268	15708

This table reports t-test results of firm-year observations in 2011 for the sample used for the DID analysis. Matching is done in 2010 as described in Section 2.4.1. Column 1 of this table analyzes mean differences of the unmatched sample (columns 1 and 2 of Table 2.1). Column 2 analyzes mean differences of the matched sample (columns 3 and 4 of Table 2.9). Liquidity shock (contin) is the size of the liquidity size as described in Section 2.3.2, total assets and employees are measured in thousands, probabilities of default are described in Section 2.3.3, ST debt to LT debt is the ratio of short-term debt to long-term debt (includes debt issued and financial debt), investment and sales growth are calculated as described in Section 2.3.1 and Section **??** respectively, EBIT lo lagged assets is earnings before interest and taxes divided by lagged total assets, Altman Z-score is measured as in Altman (1968), cash is measured in thousands and includes cash and very liquid assets. T-statistics in parentheses. * p< 0.05, ** p< 0.01, *** p< 0.001

3 How does easing Liquidity Constraints affect Aggregate Employment?

3.1 Introduction

The recent financial crisis has shown that financial shocks can have important macroeconomic effects. Contemporary macroeconomic models now routinely include "financial frictions" and "financial shocks" to explain how economic fluctuations are generated and propagated. This increased focus on financial variables has shown that financial constraints and worsening credit conditions faced by firms during recessions are extremely important to explain aggregate labor market variables, such as employment and unemployment. Two recent examples of this work are Jermann and Quadrini (2012) and Christiano et al. (2015), who use Dynamic Stochastic General Equilibirium (DSGE) models to study the macroeconomic effects of the financial shock that led into the Great Recession in the US, and find that the tightening of financial conditions faced by firms plays the main role in explaining the deterioration of economic activity and of employment.[1]

Complementing the evidence stemming from DSGE models, economic theory provides plausible mechanisms that explain the connection between credit constrained firms, employment, and unemployment. For example, Petrosky-Nadeau (2014) shows how the easing of financing constraints generates job creation in a standard search-and-matching model of equilibrium unemployment in which firms require external financing to post vacancies. Chugh (2013) shows that a model in which firms require working capital to finance their operating costs, when calibrated to the cyclical

[1] The Great Recession gave rise to an explosion in the number of papers studying financial shocks and their effects on economic fluctuations, both within and outside the DSGE tradition. Because of their sheer number we cannot possibly do justice to all of them. A necessarily arbitrary sample of recent papers on this topic includes Bassetto et al. (2015), Beck et al. (2014), Christiano et al. (2014), Liu and Minford (2014), and Meeks (2012). The main takeaway from these papers is that financial frictions and financial shocks matter for economic fluctuations. Considering a longer time frame, Schularick and Taylor (2012) show that credit growth has been a powerful predictor of financial crises in the period 1870–2008 and that these crises have had sizable output costs.

nature of financial conditions, generates large fluctuations of labor market quantities.[2] In this paper, we complement the existing theoretical literature by empirically measuring how easing credit constraints affects aggregate employment variables in a recessionary environment.

Our specific question is if—and how—a sudden and unexpected liquidity injection to the non-financial private sector affects aggregate employment. To answer this question we use Spain in the Great Recession as our laboratory. In early 2012, the Spanish central government announced that it would pay all invoices in arrears owed by sub-national governments at the regional and municipality level. This program was large (it amounted to almost 3% of Spanish GDP), and unexpected: it was first mentioned in the press in January 2012.

Municipalities were exposed to the liquidity shock to a different degree. We therefore identify the effect of the liquidity shock on employment from the cross-sectional variation in municipal employment data by comparing municipalities that received a high liquidity shock to those that received a low liquidity shock and how they fared before and after 2012. We track the evolution of employment over a window of two years prior to and after the shock, i.e., from 2010 to 2014. To take into account potential selection bias due to the heterogeneity of the size of liquidity shocks across municipalities we use a sample where we match on covariates.

3.2 Literature Review

Delgado Téllez et al. (2015) studied the impact of the repayment of sub-national debt in arrears in Spain from an aggregate perspective. They use total payments made to cancel commercial debt by municipalities and regional governments and construct a quarterly time series of payments from the public sector to the private sector. In a first exercise they estimate a VAR specification and find that the reduction of commercial arrears is associated with a cumulative GDP growth of 0.55 percentage points over the period 2012–2014. In a second exercise they use the Quarterly Model of Banco de España (MTBE, *Modelo Trimestral del Banco de España*), a large-scale macro-econometric model used for medium term macroeconomic forecasting of the Spanish economy. They find that repayment of commercial debt in arrears could account for growth in GDP on the level of between 0.3 and 0.6 cumulative percentage points over the period 2012–2014, depending on the degree to which the shock was anticipated. GDP growth in the model is explained mainly through a rise in household consumption and private investment. They also report the estimated effect on employment growth and put that figure between 0.4 and 0.7 percentage points (cumulatively over the period 2012–2014).

[2]Wasmer and Weil (2004) prove, in general, that in a model with endogenous search in credit and labor markets, credit frictions amplify macroeconomic volatility through a financial accelerator.

Other prior research has also shown a connection between financial constraints and the labor market. In a cross-country study, Borsi (2015) finds that private credit contractions have a sizable impact on the unemployment rate in OECD countries, in particular in the first two years after a disruption of credit markets. Kannan (2012) finds that stressed credit conditions constrain the pace of recovery after a recession. For the case of Spain, Bentolila et al. (2013) use Spanish firm-level data to argue that the weakest banks during the Great Recession caused a reduction in credit supply, and also on employment. Our paper complements their findings by studying liquidity that is not provided by the financial sector.

In related research, Brückner and Tuladhar (2014) estimate local government spending multipliers using annual data for 47 Japanese prefectures during a financial crisis in the 1990s. They break down government spending into different categories and find that transfers to firms in the form of credit guarantees for small and medium-sized enterprises provide the strongest effect on output. Their findings also suggest a positive effect on the labor market: they find that government transfers to firms also have a significant positive effect on employment and hours worked. Their identification relies on within-prefecture variation in government expenditures and uses a system-GMM estimation with lagged variables to mitigate endogeneity arising from reverse causality. In contrast, our identification strategy exploits the unexpectedness and differential size of the liquidity shock.

Other research related to ours is that of Corbi et al. (2014), who use a 'fuzzy' regression discontinuity design to study the effect of federal transfers on local economic activity in Brazilian municipalities. Using local GDP measures as their outcome variable, they find that transfers from the federal government tend to be more stimulative in regions with a lower penetration of bank branches, which they interpret as a proxy for tighter financial constraints. Although the lifting of financial constraints was not the main focus, several studies have studied a related question of how stimulus spending from the American Recovery and Reinvestment Act (ARRA) affected US employment using subnational geographic regions. Wilson (2012) provides an example of this kind of work.

Turning to firm-level data, Dharmapala et al. (2011b) studied a tax holiday for the repatriation of foreign earnings which they interpreted as an alleviation of financial constraints. In their sample covering multinational companies they did not find any impact on employment.

3.3 Spain in the Great Recession: Context and Institutional Detail

The Great Recession took a heavy toll on the Spanish labor market and, as argued by Campos and Reggio (2015b), the unemployment rate may have fed back into domestic demand through its effect on consumption. According to Pissarides (2013), the rise in unemployment in Spain was an outlier when compared to other OECD countries. It is exceptionally high when compared to the group of similar crisis-hit countries in the periphery of the Eurozone (Ireland, Greece, Portugal, Italy and Spain). The two main explanations that have been put forward for the surge in Spain's unemployment rate are the country's two-tier labor market (Bentolila et al., 2012) and the reduction of credit to firms (Bentolila et al., 2013).

As happened to other countries of the Eurozone, Spain suffered a credit crunch during the Great Recession. Access to credit was difficult, in particular for small and medium-sized firms. For example, in the second half of 2011, almost 30% of the firms interviewed in a study by the ECB (2014) reported that access to finance was the most pressing problem.[3] On the fiscal front, the recession eroded tax bases, also at the sub-national level. As regional and local governments saw their fiscal revenues drop, they started to fall behind on payments to suppliers. By December 2011, commercial debt in arrears by regional and local governments had accumulated to 3% of GDP.

Commercial arrears by sub-national governments were financially constraining suppliers. Because a majority of suppliers were local small and medium-sized companies, in policy circles it was thought that financial constraints were negatively affecting the real economy and employment (IMF, 2013). The Spanish central government responded by passing legislation on February 24 and March 9, 2012 to pay the commercial debt in arrears. It set up a new state-owned vehicle, the *Fondo para la Financiación del Pago a Proveedores* (FFPP).

The FFPP was tasked with making payments directly to suppliers who were owed money by sub-national governments. These payments were made over a two-month period in 2012 and amounted to almost 3% of Spanish GDP.[4] Legally, the stock of commercial debt previously owed to suppliers turned into financial debt now owed to the FFPP. Participation was mandatory for municipalities and voluntary for regional governments. Three out of 17 regional governments (Basque Country, Galicia and Navarre) decided not to participate in the program. In order to participate, regional governments had to commit to a fiscal adjustment plan. Because they could not opt

[3] We plot the evolution of this fraction over the period 2010–2014 in Figure 3.1.

[4] There were three payment dates: on May 28, EUR9.3bn were transferred to suppliers of municipalities; on June 25, EUR17.7bn were transferred to suppliers of the 14 participating regions; finally, on July 30, EUR 0.3bn were transferred to suppliers of municipalities that had been left out in the May payment.

out of the repayment scheme, municipalities were not required to commit to a fiscal adjustment plan although they could voluntarily do so. If their adjustment plan was approved by the central government, then they were given more favorable conditions on the debt owed to the FFPP.

Funds used to pay suppliers were guaranteed by the share in national tax receipts of each region and municipality. To repay debt to the FFPP, regions and municipalities without an approved fiscal adjustment plan would have part of their share in national taxes withheld over a 5-year period. On the other hand, if they secured approval of a fiscal adjustment plan, then the money paid on their behalf could be financed at an attractive interest rate with a 10-year loan, with a 2-year interest-only grace period. The interest rate was set at the funding cost of the Spanish Treasury plus 115 basis points, with an intermediation margin of 30 basis points. These were favorable conditions in the context of 2012.

The FFPP obtained funds from a EUR 35bn syndicated loan granted by a pool Spanish banks, including the state-owned ICO, which made the single largest contribution. This syndicated loan was guaranteed by the Spanish government. From a national accounting point of view, the liabilities of the FFPP became part of the stock of outstanding general government debt.

The payment of commercial debt in arrears through the FFPP was unexpected: it was not part of the electoral program of the Partido Popular (PP), which came into power in the November 2011 general election. As such, it provides a quasi-experimental setting to study the effect of relaxing financing constraints on employment variables. The conception, communication, and execution of the program wholly took place in 2012. The first mention of this program in the press is an article in La Vanguardia in January 2012. The legislation passed into law in February and March 2012. By July 2012, the program had been completed and the last invoices had been paid.

3.4 Data

3.4.1 Data sources

Employment and unemployment data are obtained from the standard sources. Employment is measured as the year-end number of workers by the Spanish Social Security Administration. These data are maintained by the Ministry of Employment and Social Security and contain all workers affiliated to the Social Security System and by municipality and job classification on a monthly basis from 01/2003 onward.

The number of unemployed is the year-end number of people registered as unemployed and counted as such by the Spanish Ministry of Employment and Social

Security. It contains the number of registered unemployed by gender, age group, economic sector, and municipality on a monthly basis from 05/2005 onward.

We use per-capita normalizations for employment and unemployment flows. We obtain population counts from the Continuous Census of the National Statistics Institute (INE). This data set contains the number of residents in a municipality broken up by gender and age for all Spanish municipalities on a yearly basis.

Data on the FFPP were obtained from the Instituto de Crédito Oficial (ICO), the state-owned bank that channeled the payments to the suppliers. These data include anonymized information for firms accounting for 48.2% of all suppliers that benefited from this measure (64,879 out of 134,568) and almost 70% of the funds injected by the FFPP (19 out of 27.3 billion euros). The data set includes information on the number and amount of invoices broken down by local government, the amounts seized by the government due to unpaid taxes and social contributions and the dates in which the payments took place. The difference between the amount of unpaid bills and the seized amount equals the cash the firm effectively receives. Interestingly, the data set also matches this information to to the ZIP code of firms that are paid.

The data set does not include information on 46,564 self-employed individuals (34.6% of suppliers and 1.5% of funds), nor on 23,125 firms (17.2% of suppliers and 29% of funds) that were not available at the Iberian Balance Sheet Analysis System (SABI), a database with a coverage of more than 1.25 million firms in Spain which is provided by INFORMA D&B in collaboration with Bureau Van Dijk.

Budget information on municipalities is obtained from the budget database of the Spanish Ministry of Finance and Public Administration. This database contains the annual budget of all Spanish municipalities for the years 2005 through 2014. Debt by municipality is also obtained from the Spanish Ministry of Finance and Public Administration and is available on a yearly basis from 2008 onward. The revenue of the tax on economic activities (IAE) for each municipality is obtained from yearly reports by the Spanish Ministry of Finance and Public Administration. These reports contain economic activities tax revenues on a yearly basis from 2010 onward for all municipalities with the exception of the Basque Country and Navarre.

3.4.2 Description of the liquidity shock

We normalize liquidity received by each municipality i in 2012 by the working-age population of that municipality in 2011. Per-capita liquidity ℓ_i is defined as

$$\ell_i = \frac{\text{Liquidity injection to municipality } i \text{ in 2012}}{\text{Population aged 15–64 in municipality } i \text{ in 2011}}. \tag{3.1}$$

Figure 3.2 shows the per-capita size of shock by quartiles measured at the location of the local administration that had the commercial debt. We call this the origin of the liquidity shock. Figure 3.3 shows the per-capita size of shock by quartiles measured at the location of the legal address of the supplier. We call this the destination of the shock.

At first sight, these figures show that the origin of the shock resembles the destination of the shock. Geographically, there is a diagonal strip that passes North of Madrid where the liquidity shock is weaker or non-existent. This corresponds to municipalities that are very small in size and in population. One of the differences between the two figures shows up when we turn to the destination of the liquidity shock. The destination of the funds is more concentrated in large and populous areas compared to the origin of the shock.

The first two columns in Tables 3.1 and 3.2 show the characteristics of municipalities that received liquidity versus those that did not. Because municipalities that do not receive liquidity strongly differ in their characteristics from those that do (e.g., they are smaller and less unpopulated), they are ill-suited for counterfactual experiments. Controlling for the set of characteristics that are observed might be a questionable approach to overcome selection bias because there are likely unobserved factors that cannot be accounted for in this case. For this reason, we exclude municipalities with no exposure to the liquidity shock from the analysis in what follows.

Next, we turn to the size of the liquidity injection. We classify all municipalities that were exposed to a liquidity shock into two groups according to the magnitude of per-capita liquidity they received. We set the threshold at the median, i.e., 50% of municipalities fall into the low and high groups. The last two columns in Tables 3.1 and 3.2 compare the characteristics of municipalities that were exposed to a low versus a high per-capita liquidity injection. Gaps in characteristics become smaller, often by an order of magnitude, relative to the prior analysis that compared no-shock versus shock municipalities. However, municipalities in the low and high groups still differ in some dimensions. That is why in our later analysis we use matching on covariates to construct a comparable control group.

3.5 Empirical Strategy

Our unit of observation is a municipality and we use yearly data for the years 2010–2014. This gives us a 2-year window before and after the liquidity shock that occurred in 2012. The effect on employment and unemployment (either through direct hiring or through spillover effects) can be located either in the municipality that owed money to a firm (where the money originates), or in the municipality where the firm has its headquarters (the destination of the money). We therefore explore the effects on employment and unemployment at both the origin and the destination of the funds.

In our main analysis we exclude municipalities that did not receive FFPP funds. The reason is that municipalities that did not receive funds are very different from the rest of the population. These differences fall into two categories. First, non-recipient municipalities are typically small, sparsely populated, and rural. Thus, comparisons between municipalities with positive liquidity shocks and a zero shock do not plausibly lead to an effect that can be argued to be causal. Second, when focusing at the origin of the money, municipalities from the Basque Country and Navarre were excluded from participating in the FFPP because these two regions enjoy a special tax status. Any effect derived from a comparison between municipalities within and outside these regions cannot be disentangled from the special fiscal status.

In Section 3.4.2 we compare recipient to non-recipient municipalities to show how they differ on certain dimensions, such as population, labor market variables, and fiscal variables. Our results presented in Section 3.6 rely exclusively on municipalities exposed to a positive liquidity shock. This population is more homogeneous and selection bias is less of an issue. In any case, we go to great lengths to rule out selection bias and other confounding factors.

3.5.1 Outcome variables

We use two different labor market measures: employment and unemployment. Our first outcome variable consists of employment flows normalized by working-age population in 2011, the same variable we used to normalize the liquidity variable. Employment flows are constructed for each municipality i as

$$\Delta e_{it} \equiv \frac{E_{i,t} - E_{i,t-1}}{N_{i,15-64}} \times 100, \tag{3.2}$$

where $E_{i,t-1}$ and $E_{i,t}$ are employment counts in two consecutive years and $N_{i,15-64}$ is the working-age population in 2011, defined as the population aged between 15 and 64. Likewise, letting $U_{i,t-1}$ and $U_{i,t}$ denote the stock of unemployed in two consecutive years, unemployment flows in a municipality are defined as

$$\Delta u_{it} \equiv \frac{U_{i,t} - U_{i,t-1}}{N_{i,15-64}} \times 100. \tag{3.3}$$

As we explain in Section 3.4.1, employment and unemployment counts are obtained from public sources: two different datasets maintained by the Spanish Ministry of Employment and Social Security. Population data are obtained from the Spanish National Statistics Institute.[5]

3.5.2 Specification

3.5.2.1 Benchmark specification

We use i to index municipalities, t to index time, and $y_{it} \in \{\Delta e_{it}, \Delta u_{it}\}$ to refer to any of the outcome variables of interest. Letting ℓ_i stand for the amount of per-capita liquidity received in 2012 under the FFPP program in municipality i, we estimate the parameter λ in the regression:

$$y_{it} = \alpha_i + \lambda \ell_i \times I_{\{t>2012\}} + \delta_t + X_{it}\beta + \varepsilon_{it}, \quad t \neq 2012. \tag{3.6}$$

The interaction term $\lambda \ell_i \times I_{\{t>2012\}}$ captures the effect of interest. Time dummy variables δ_t allow for arbitrary common time variation. X_{it} is a vector of time-varying control variables that aim to capture heterogeneity in the evolution of employment and unemployment across municipalities. In our main estimation we allow for municipality-specific fixed effects α_i and estimate this equation using the standard fixed-effects within-estimator. We also experiment with a pooled-OLS estimation, i.e., one that imposes

[5] As a robustness exercise, we experimented with a second way of measuring outcome variables through the use of symmetric growth rates. The symmetric growth rate of employment is defined by

$$g_t^E \equiv \frac{E_t - E_{t-1}}{\frac{1}{2}E_t + \frac{1}{2}E_{t-1}} \times 100 \tag{3.4}$$

and the symmetric growth rate of unemployment is defined by

$$g_t^U \equiv \frac{U_t - U_{t-1}}{\frac{1}{2}U_t + \frac{1}{2}U_{t-1}} \times 100. \tag{3.5}$$

It can be shown from the definition that the symmetric growth rate is bounded in the range $[-200, 200]$. The symmetric growth rate is used mainly in the literature using establishment-level employment microdata (see, e.g., Davis et al., 1998). It is a second-order approximation of the log difference growth rate around zero. The symmetric growth rate's main advantage over the usual growth rate is that it is robust to the presence of outliers, which may pose problems in micro datasets. All our results held for outcome variables measured in this way.

$\alpha_i = \alpha$ for all i. In this case, we add a term $\gamma \ell_i$ to the right hand side of (3.6) and also increase the number of variables in X_{it}, which can contain variables that are constant by municipality. In all estimations, we use panel-robust standard errors.

Our standard set of control variables consists of population, budget variables at the municipality level (per-capita income, per-capita expenditure and per-capita debt), and the political landscape, measured as the number of assembly members (*concejales*) in the local elections in 2007 and 2011 belonging to the three main political parties at the time (PP, PSOE, IU). We also add an economic activity indicator, defined as per-capita revenue of the economic activity tax (IAE) collected by municipalities. This variable is commonly used as an indicator of economic activity at the municipal level, for example in the influential yearly report by *La Caixa*.[6]

We include interaction terms between time dummies and dummies for regions (CCAA). These interaction terms will capture any time-varying effects that are common by CCAA. The reason for including these terms is that at the same time of the FFPP plan to municipalities, there was similar plan designed for CCAA. In addition, because the liquidity injection occurred during 2012 it is unclear whether this year is already affected by the liquidity injection. We decided to exclude this year from our estimations in the benchmark equation. However, as we show below, the year 2012 can be included in a more flexible model.

3.5.2.2 Flexible specification

Because we have two periods prior to the liquidity injection and two periods after it, we can estimate a more flexible specification. As argued by Mora and Reggio (2012) and Mora and Reggio (2015), there are advantages to replacing the dummy variable indicating the period before and after the occurrence of the shock with a more flexible specification. Their argument is made for binary treatment variables but the intuition carries over to a our continuous variable measuring liquidity.

In the flexible specification the single interaction term $\lambda \ell_i \times I_{\{t>2012\}}$ in (3.6) is replaced with four yearly interaction terms, so that the equation to be estimated becomes

$$y_{it} = \alpha_i + \sum_{\tau=2011}^{2014} \lambda^{\tau} \ell_i \times I_{\{t=\tau\}} + \delta_t + X_{it}\beta + \varepsilon_{it}. \tag{3.7}$$

Otherwise, we include the same controls as those of our benchmark specification.

Our interest in this specification lies in the estimation of the coefficients λ^{2011}, λ^{2012}, λ^{2013}, and λ^{2014} (the year 2010 is the excluded category). This flexible specification has

[6] In this report, La Caixa reports data only for the largest 3,245 municipalities out of a total of roughly 8,100 municipalities in Spain. We therefore obtain IAE tax revenue by municipality directly from yearly reports by the Spanish Ministry of Finance and Public Administration, as explained in our section on data sources.

a number of advantages over the standard specification in (3.6). As argued by Mora and Reggio (2012), our specification does not directly impose a common-trend assumption on labor market variables before 2012. In fact, the flexible specification allows to test whether the years 2010 and 2011 differ in terms of the evolution of labor market variables by performing a simple t-test on the coefficient estimated for λ^{2011}.

There are two additional advantages from the flexible model. The first advantage is that the year 2012 can be included without having to decide whether it is affected by the shock. The data are allowed to speak for themselves. The second advantage is that the effect of the liquidity injection is not constrained to be constant over the whole treatment period, i.e., this flexible model allows for the possibility that $\lambda^{2013} \neq \lambda^{2014}$.

3.5.2.3 Binary specification

In addition to the continuous variable ℓ_i, we also estimate specifications of the form

$$y_{it} = \alpha_i + \lambda_H I_{\{\ell_i \in L_H\}} \times I_{\{t>2012\}} + \delta_t + X_{it}\beta + \varepsilon_{it}, \quad t \neq 2012, \tag{3.8}$$

and

$$y_{it} = \alpha_i + \sum_{\tau=2011}^{2014} \lambda_H^\tau I_{\{\ell_i \in L_H\}} \times I_{\{t=\tau\}} + \delta_t + X_{it}\beta + \varepsilon_{it}, \tag{3.9}$$

where $I_{\{\ell_i \in L_H\}}$ is an indicator of whether liquidity ℓ in municipality i belongs to a group of high liquidity recipients. We considered different definitions of the group L_H, such as the top half of the sample, the top quartile, etc.

The drawback of this approach is that by transforming our variable of interest into a discrete variable with broad categories the estimation may be losing precision. The advantage of the approach is that we can use standard tools derived for binary treatment variables to study whether heterogeneity in the covariate distribution is biasing the estimated effect of the liquidity shock.

3.5.2.4 Matching on covariates

As highlighted by Imbens (2004), the estimation of average treatment effects is sensitive to differences in the covariate distribution. The specifications of the form (3.8) and (3.9) allow us to perform matching on covariates. We use propensity score matching to select a single match for each of the municipalities in the high group L_H. According to Imbens and Wooldridge (2009), this leads to credible inference with the least bias, at the cost of sacrificing some precision. Matching is done with replacement, so that the same municipality outside of the high group L_H can perform as a match for more than one observation in the L_H group. We match on covariates for the year 2010. As a general rule, we use variables in levels and add their squares only if they improve the results of the the matching algorithm

For our matching variables we used the logarithm of the population, budget variables, and the per-capita tax on economic activity. In addition to these variables we also included the geographical location of each municipality represented as the centroid of the 2-dimensional coordinates of each municipality on a map in geospatial vector data format. As argued by Heckman et al. (1998) geographically-matched controls greatly reduce the potential selection bias, especially in the presence of heterogeneous effects. We found that matching for the destination of funds was greatly improved by also adding the employment rate measured in 2010. The results of the matching procedure can be seen in Section 3.8.5. Despite the reduced set of variables that were used in the matching procedure, we observe that the match is very good for the year 2010, and also for the year 2011.

We estimate the specification for the binary treatment variable in (3.8) and (3.9) for the original sample and for the sample matched on covariates. Our estimations include the same controls as those of our continuous variable specification.

3.6 Results

Our results using the empirical strategy laid out in Section 3.5 suggest that liquidity provision had significant effects on unemployment and employment both at the origin of the liquidity injection and at the destination of of liquidity. Our results are presented in a series of tables shown in the Appendix. The main conclusion is that the effect on unemployment is stronger at the origin than at the destination whereas the effect on employment is stronger at the destination. Moreover, there is evidence indicating that the effect on employment carries over into 2014 at the destination.

3.6.1 The fully flexible specification

The effect on unemployment is reported in two tables. Table 3.3 measures the liquidity shock at the location where suppliers were owed the money, i.e., at municipalities that had their invoices in arrears repaid through the FFPP program. Table 3.4 measures the location at the destination of funds, the municipality where suppliers had their legal address. Coefficients in columns 1 and 2 are multiplied by 1,000. Therefore, they measure changes in the number of unemployed individuals per EUR 1,000 that flow into a municipality.

We are most interested in the results of the continuous flexible specification (Column 2) and the binary specification with the matched sample (Column 6).

According to the continuous specification there is a significant effect on unemployment only at the origin of funds. In fact, this effect is negative and significantly different from zero only in 2013. At the destination there does not seem to be any effect in the continuous specification.

Things change in the binary specification (Column 4). These results for the whole sample show a significantly negative effect on unemployment in 2013 and 2014, if the location of the origin is considered. When we use the sample that is matched on covariates (Column 6), the significance is reduced although point estimates remain virtually unchanged.

The first look at unemployment in the fully flexible model suggests that the effect is clearer at the origin of funds.

Notably, the coefficients on λ^{2011} are not significantly different from zero. We therefore later fit a reduced model that imposes $\lambda^{2010} = \lambda^{2011}$.

Before doing so, we turn to employment. Table 3.5 focuses on the origin and Table 3.6 on the destination of funds.

The continuous specification (Column 2) shows significant positive effects on employment in 2013 at the origin and in 2013 and 2014 at the destination. The estimation at the origin also shows a large point estimate for 2012 which is not significantly different from zero. The effect measured at the destination seems to be stronger and more persistent than at the origin.

This overall result is maintained when we turn to the binary specification, although significance is reduced. Focusing on the matched sample (Column 6), we find that at the destination the matching procedure has eliminated the large and significant coefficient on λ^{2011}. Again, this is evidence in favor of a more parsimonious model in which $\lambda^{2010} = \lambda^{2011}$.

We now turn to a reduced model that imposes $\lambda^{2010} = \lambda^{2011}$.

3.6.2 A flexible specification with $\lambda^{2010} = \lambda^{2011}$

Results of imposing $\lambda^{2010} = \lambda^{2011}$ are shown in Tables 3.7 through Tables 3.10. This specification yields results that are qualitatively similar to those for the fully flexible model. However, the precision with which the coefficients are estimated is larger.

There are two main conclusions from these results. First, the evidence for a reduction of unemployment is stronger at the origin whereas the evidence for an increase of employment is stronger at the destination. Second, there are differences in timing. Effects do not persist into 2014 in the same way at the origin and at the destination.

3.6.3 Robustness checks

We conducted several robustness checks. In addition to the fixed-effects specification, we estimated all our equations using a pooled-OLS estimator (clustering standard errors by municipality). We obtained results that were qualitatively similar although, in general, with smaller estimated standard errors, and therefore higher statistical significance. In this sense, our fixed-effects results are the more conservative choice.

We conducted several robustness checks on our matching procedure, as well. We changed the caliper from our preferred value of 0.005 to smaller and larger values and obtained overall similar results. We also explored using other variables in the matching procedure but the use of groups of variables that delivered a fit comparable with our preferred case delivered similar results.

3.7 Conclusion

We find that the liquidity injection had effects both at originating and destination municipalities. However, the size and timing of the effects are different. At the origin, there was a strong reduction in 2013 that persists until 2014. At the destination, the effect is weaker and concentrated in 2013. In comparison, the effect on employment was stronger and more persistent at the destination.

Our preliminary findings suggest that the liquidity injection in 2012, and the elimination of financial constraints it implied, had a plausibly causal effect on labor market outcomes. Our results are a first step in attempting to study the effect of the FFPP program on employment and unemployment. The objective is to obtain estimates that can be argued to be of a causal nature. For this, further work is needed in order to weed out potential selection bias.

More generally, our findings address the effect of “financial frictions” and “financial shocks” on employment that were brought to the highlight by the recent financial crisis, and which are now routinely used in economic models. We do so by using micro data at the municipality level. Our preliminary findings suggest that the strong effects on the real economy predicted by economic theory are validated once the geographical origin and destination of funds is considered.

3.8 Figures and Tables

Figure 3.1 *Fraction of small and medium-sized firms in Spain mentioning access to finance as the most pressing problem over the period 2010–2014 (constructed from the ECB series SAFE.H.ES.SME.A.0.0.0.Q0.ZZZZ.P3.AL.WP)*

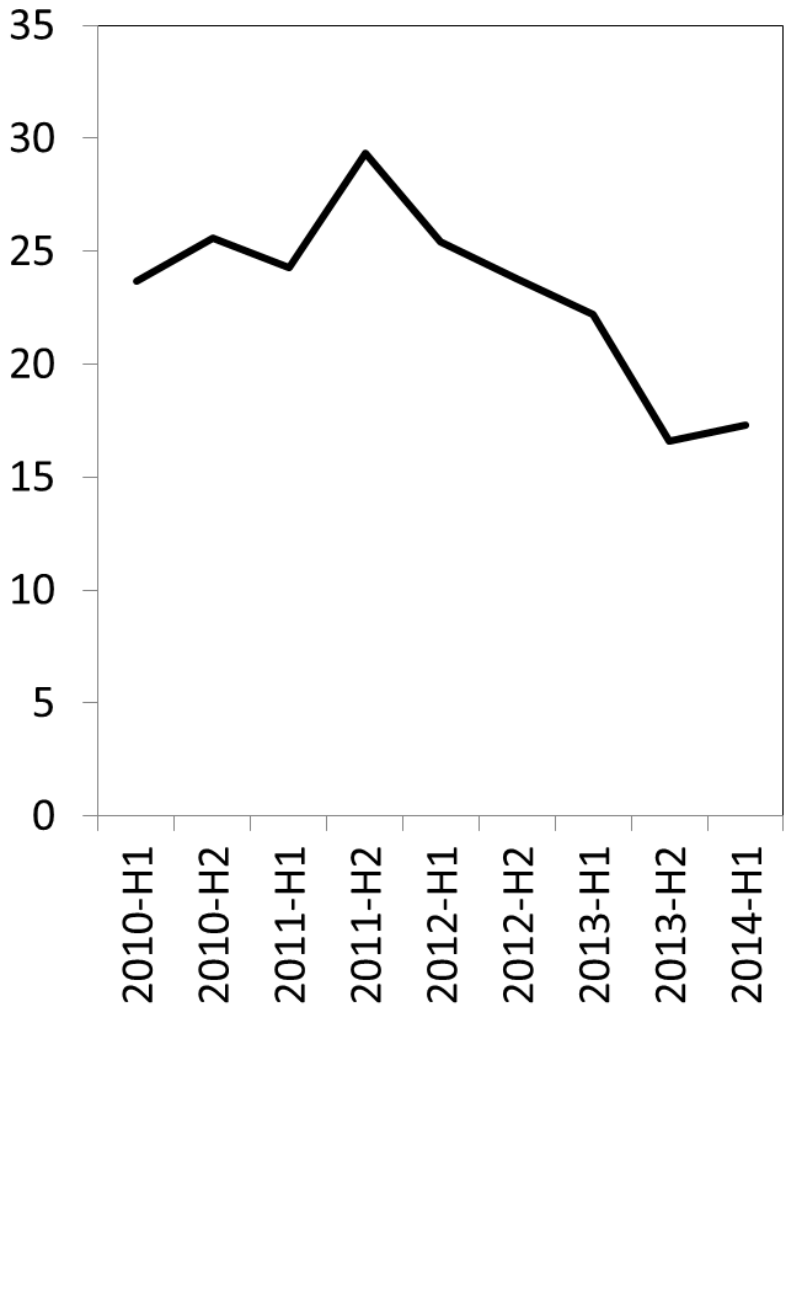


Figure 3.2 *Per-capita size of shock by quartiles measured at the location of the local administration that had the commercial debt (Origin). Canary Islands are not included to preserve the scale.*

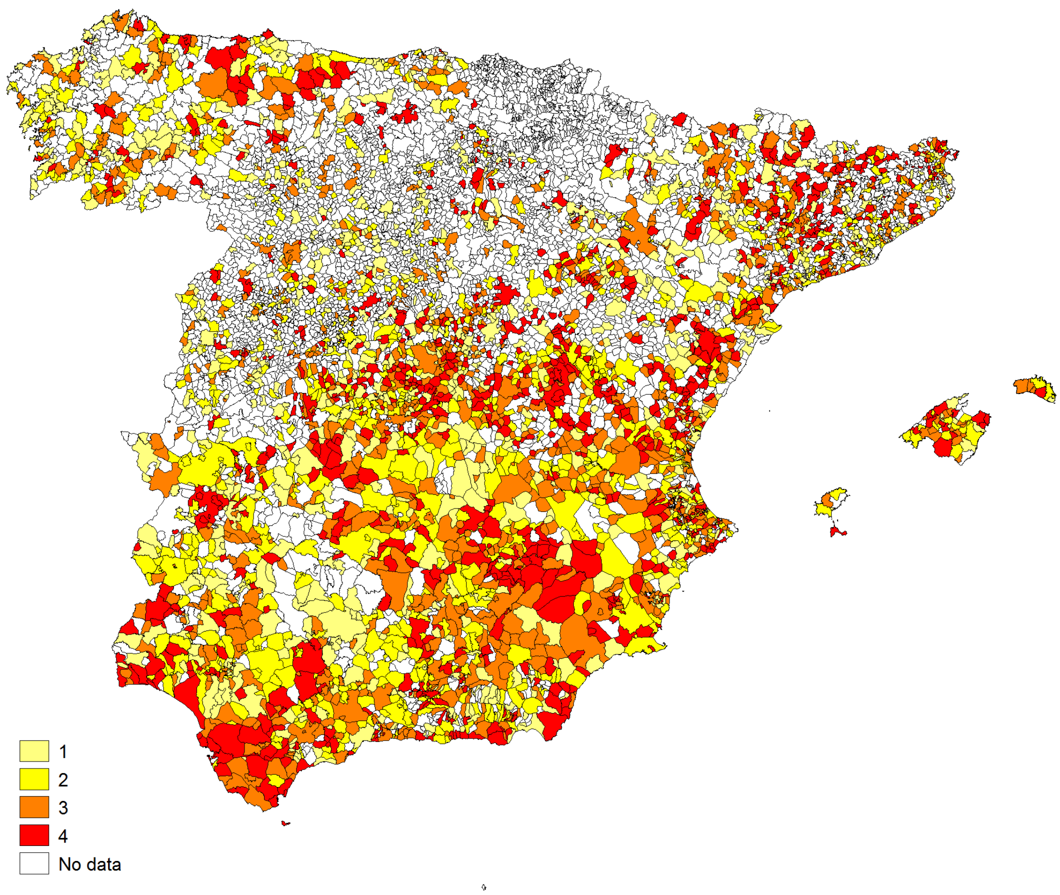


Figure 3.3 *Per-capita size of shock by quartiles measured at the location of the legal address of the supplier (Destination). Canary Islands are not included to preserve the scale.*

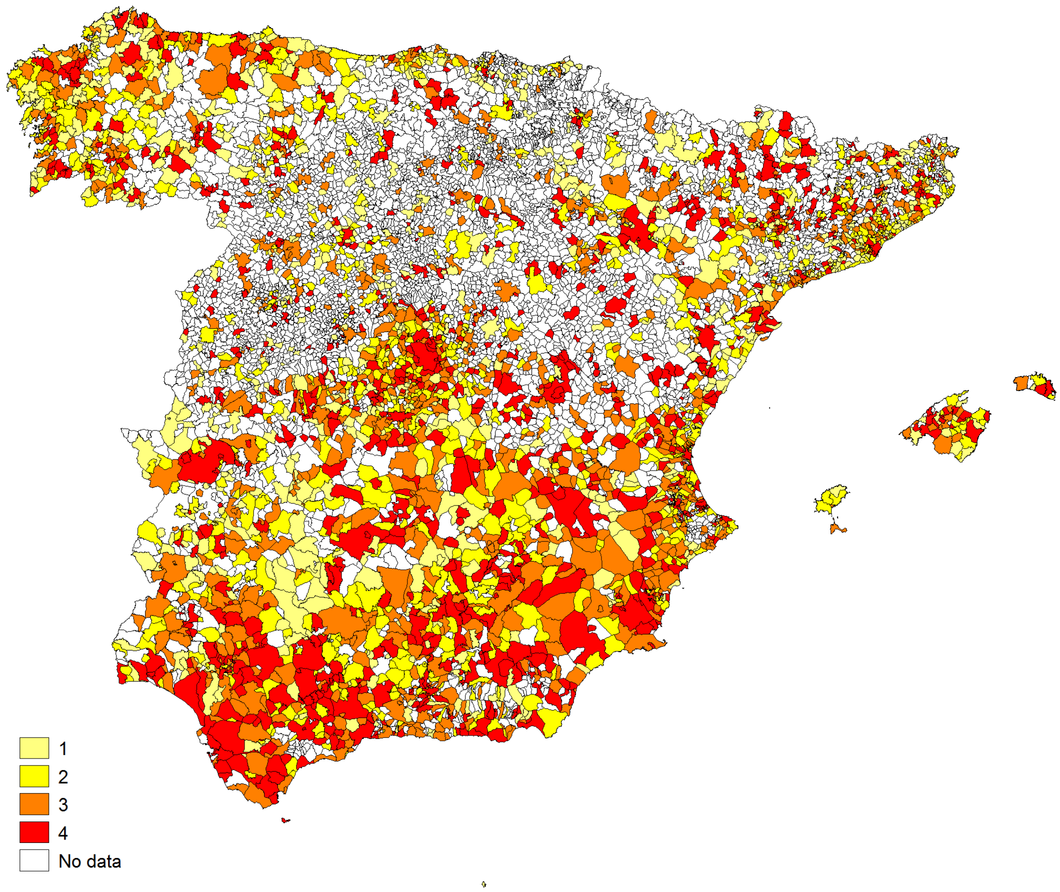


Table 3.1 *Summary Statistics (origin: municipality where funds are paid).*

	No Shock	Shock	Low	High
Per-capita FFPP injection	0.00	527.17	132.31	922.44
	(0.00)	(854.68)	(86.65)	(1068.69)
Population	2650.00	9393.11	10085.54	8699.95
	(27105.27)	(62892.03)	(36406.21)	(81174.62)
Unemployment rate	0.08	0.12	0.12	0.12
	(0.05)	(0.05)	(0.05)	(0.05)
Employment rate	0.42	0.39	0.40	0.38
	(0.35)	(0.19)	(0.18)	(0.19)
Fraction of population aged 15-64	0.60	0.63	0.64	0.63
	(0.09)	(0.07)	(0.06)	(0.07)
Percentage PSOE	0.29	0.35	0.35	0.35
	(0.23)	(0.19)	(0.18)	(0.20)
Percentage PP	0.41	0.38	0.38	0.38
	(0.30)	(0.22)	(0.21)	(0.23)
Percentage IU	0.02	0.04	0.05	0.04
	(0.07)	(0.09)	(0.10)	(0.09)
Per-capita IAE (EUR/N)	73.69	30.37	28.09	32.66
	(365.27)	(192.02)	(89.90)	(256.32)
Per-capita debt (EUR/N)	294.45	513.49	414.79	612.29
	(717.72)	(592.68)	(471.29)	(679.13)
Per-capita revenue (EUR/N)	2560.45	1938.81	1678.11	2200.48
	(3166.87)	(1428.03)	(924.00)	(1758.83)
Per-capita expenditure (EUR/N)	2501.77	1981.47	1701.21	2262.77
	(2435.56)	(1473.06)	(936.25)	(1819.66)
Observations	4326	3784	1892	1892

This table reports summary statistics of municipality-year observations in 2011 for the entire sample. The data is classified according to the origin of the funds, this is, regarding the municipality that owed the arrears to the firms. Municipalities are classified as "No Shock" (column 1) if they do not participate in the FFPP, and they are classified as "Shock" (column 2) if they do participate in the FFPP. Municipalities in column 2 are divided into two groups according to the median per-capita liquidity they received, the lower group results appear in column 3 and the higher group results appear in column 4. Per-capita FFPP injection is measured as the liquidity injected in a municipality divided by working age population. Population is the amount of people registered in the census of the municipality. Unemployment rate is the ratio of unemployed population to working age population. Employment rate is the ratio of employed population to working age population. Fraction of population aged 15-64 is the percentage of population aged 15-64. Percentage PSOE, Percentage PP and Percentage IU are the percentage of total votes received by PSOE, PP and IU in the 2011 municipal elections. Per-capita IAE, Per-capita debt, Per-capita revenue and Per-capita expenditure measure the municipal revenue from the IAE (tax on economic activities), the municipal debt, total municipal revenues and total municipal expenditures normalized by working age population. Numbers reported are cross-sectional averages and standard errors in parentheses. Number of observations appear in the last row.

Table 3.2 *Summary Statistics (destination: municipality where firms are headquartered).*

	No Shock	Shock	Low	High
Per-capita FFPP injection	0.00	158.26	13.43	303.18
	(0.00)	(678.50)	(10.85)	(937.62)
Population	757.29	12758.99	7618.61	17902.40
	(2566.44)	(72550.32)	(18973.31)	(100599.03)
Unemployment rate	0.08	0.12	0.12	0.12
	(0.05)	(0.04)	(0.04)	(0.05)
Employment rate	0.38	0.44	0.42	0.46
	(0.30)	(0.26)	(0.24)	(0.29)
Fraction of population aged 15-64	0.59	0.65	0.65	0.65
	(0.09)	(0.05)	(0.05)	(0.06)
Percentage PSOE	0.31	0.33	0.33	0.33
	(0.24)	(0.18)	(0.18)	(0.18)
Percentage PP	0.43	0.36	0.35	0.37
	(0.29)	(0.21)	(0.22)	(0.21)
Percentage IU	0.02	0.05	0.04	0.05
	(0.07)	(0.10)	(0.09)	(0.10)
Per-capita IAE (EUR/N)	59.10	43.00	39.53	46.40
	(288.66)	(297.85)	(148.81)	(392.12)
Per-capita debt (EUR/N)	325.88	494.43	458.42	530.45
	(727.72)	(570.10)	(504.28)	(627.19)
Per-capita revenue (EUR/N)	2702.09	1674.52	1635.11	1713.96
	(3155.50)	(933.24)	(796.59)	(1051.13)
Per-capita expenditure (EUR/N)	2673.17	1687.32	1640.15	1734.52
	(2504.43)	(938.40)	(775.39)	(1075.26)
Observations	4705	3405	1703	1702

This table reports summary statistics of municipality-year observations in 2011 for the entire sample. The data is classified according to the destination of the funds, this is, regarding the headquarters of the firms. Municipalities are classified as "No Shock" (column 1) if they do not participate in the FFPP, and they are classified as "Shock" (column 2) if they do participate in the FFPP. Municipalities in column 2 are divided into two groups according to the median per-capita liquidity they received, the lower group results appear in column 3 and the higher group results appear in column 4. Per-capita FFPP injection is measured as the liquidity injected in a municipality divided by working age population. Population is the amount of people registered in the census of the municipality. Unemployment rate is the ratio of unemployed population to working age population. Employment rate is the ratio of employed population to working age population. Fraction of population aged 15-64 is the percentage of population aged 15-64. Percentage PSOE, Percentage PP and Percentage IU are the percentage of total votes received by PSOE, PP and IU in the 2011 municipal elections. Per-capita IAE, Per-capita debt, Per-capita revenue and Per-capita expenditure measure the municipal revenue from the IAE (tax on economic activities), the municipal debt, total municipal revenues and total municipal expenditures normalized by working age population. Numbers reported are cross-sectional averages and standard errors in parentheses. Number of observations appear in the last row.

3.8.1 Results for Unemployment (Fixed Effects)

Table 3.3 *Effect on Δu (Origin).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Binary	(4) Binary Flex	(5) Matched	(6) Matched Flex
$\lambda^{\geq 2013}$	-0.104*					
	(0.059)					
λ^{2011}		-0.076				
		(0.098)				
λ^{2012}		-0.099				
		(0.082)				
λ^{2013}		-0.193**				
		(0.088)				
λ^{2014}		-0.103				
		(0.104)				
$\lambda_H^{\geq 2013}$			-0.182***		-0.210**	
			(0.058)		(0.084)	
λ_H^{2011}				-0.129		-0.072
				(0.107)		(0.161)
λ_H^{2012}				-0.134		-0.105
				(0.097)		(0.140)
λ_H^{2013}				-0.264***		-0.260*
				(0.096)		(0.148)
λ_H^{2014}				-0.240**		-0.233
				(0.097)		(0.145)
Observations	14,671	18,339	14,671	18,339	13,995	17,494
R-squared	0.207	0.219	0.207	0.219	0.197	0.210
Number of id	3,668	3,668	3,668	3,668	2,685	2,685

This table presents estimates from panel regressions explaining unemployment change where the liquidity injection originates for the period 2010 to 2014. The dependent variable is unemployment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.1. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted yearly dummy is 2010. $\lambda^{2011}, \lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2011, 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2011}, λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2011, 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

Table 3.4 *Effect on Δu (Destination).*

	(1)	(2)	(3)	(4)	(5)	(6)
VARIABLES	Benchmark	Flexible	Binary	Binary Flex	Matched	Matched Flex
$\lambda^{\geq 2013}$	-0.043					
	(0.053)					
λ^{2011}		0.049				
		(0.086)				
λ^{2012}		0.077				
		(0.139)				
λ^{2013}		-0.118				
		(0.098)				
λ^{2014}		0.073				
		(0.115)				
$\lambda_H^{\geq 2013}$			-0.078*		-0.138**	
			(0.047)		(0.061)	
λ_H^{2011}				0.023		0.073
				(0.084)		(0.105)
λ_H^{2012}				-0.004		0.056
				(0.077)		(0.126)
λ_H^{2013}				-0.025		-0.135
				(0.076)		(0.101)
λ_H^{2014}				-0.109		-0.073
				(0.074)		(0.099)
Observations	13,087	16,359	13,087	16,359	12,824	16,030
R-squared	0.307	0.327	0.307	0.327	0.302	0.312
Number of id	3,272	3,272	3,272	3,272	2,453	2,453

This table presents estimates from panel regressions explaining unemployment change in the municipality where the firms that receive the liquidity injection are located for the period 2010 to 2014. The dependent variable is unemployment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.1. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted yearly dummy is 2010. $\lambda^{2011}, \lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2011, 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2011}, λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2011, 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

3.8.2 Results for Employment (Fixed Effects)

Table 3.5 *Effect on Δe (Origin).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Binary	(4) Binary Flex	(5) Matched	(6) Matched Flex
$\lambda^{\geq 2013}$	0.102					
	(0.093)					
λ^{2011}		0.180				
		(0.123)				
λ^{2012}		0.171				
		(0.116)				
λ^{2013}		0.232*				
		(0.126)				
λ^{2014}		0.161				
		(0.137)				
$\lambda_H^{\geq 2013}$			0.166		0.208	
			(0.128)		(0.173)	
λ_H^{2011}				-0.096		-0.098
				(0.212)		(0.277)
λ_H^{2012}				0.314		0.307
				(0.206)		(0.280)
λ_H^{2013}				0.331*		0.342
				(0.196)		(0.248)
λ_H^{2014}				-0.080		-0.015
				(0.183)		(0.229)
Observations	14,605	18,257	14,605	18,257	13,915	17,401
R-squared	0.053	0.079	0.053	0.080	0.060	0.085
Number of id	3,660	3,661	3,660	3,661	2,680	2,681

This table presents estimates from panel regressions explaining employment change where the liquidity injection originates for the period 2010 to 2014. The dependent variable is employment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.1. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted yearly dummy is 2010. $\lambda^{2011}, \lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2011, 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2011}, λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2011, 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

Table 3.6 *Effect on Δe (Destination).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Binary	(4) Binary Flex	(5) Matched	(6) Matched Flex
$\lambda^{\geq 2013}$	0.511**					
	(0.242)					
λ^{2011}		-0.044				
		(0.373)				
λ^{2012}		-0.232				
		(0.209)				
λ^{2013}		0.458**				
		(0.212)				
λ^{2014}		0.520**				
		(0.246)				
$\lambda_H^{\geq 2013}$			0.357***		0.460**	
			(0.126)		(0.199)	
λ_H^{2011}				-0.494**		-0.174
				(0.206)		(0.248)
λ_H^{2012}				-0.199		0.176
				(0.225)		(0.294)
λ_H^{2013}				-0.166		0.334
				(0.199)		(0.264)
λ_H^{2014}				0.386**		0.412*
				(0.190)		(0.249)
Observations	13,065	16,330	13,065	16,330	12,821	16,026
R-squared	0.074	0.101	0.073	0.100	0.084	0.116
Number of id	3,270	3,270	3,270	3,270	2,453	2,453

This table presents estimates from panel regressions explaining employment change in the municipality where the firms that receive the liquidity injection are located for the period 2010 to 2014. The dependent variable is employment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.1. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted yearly dummy is 2010. $\lambda^{2011}, \lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2011, 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2011}, λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2011, 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

3.8.3 Results for Unemployment (Fixed Effects, $\lambda^{2010} = \lambda^{2011}$)

Table 3.7 *Effect on Δu (Origin).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Matched	(4) Matched Flex
$\lambda^{\geq 2013}$	-0.104*			
	(0.059)			
λ^{2012}		-0.061		
		(0.058)		
λ^{2013}		-0.155**		
		(0.062)		
λ^{2014}		-0.064		
		(0.074)		
$\lambda_H^{\geq 2013}$			-0.210**	
			(0.084)	
λ_H^{2012}				-0.069
				(0.112)
λ_H^{2013}				-0.224*
				(0.117)
λ_H^{2014}				-0.198*
				(0.111)
Observations	14,671	18,339	13,995	17,494
R-squared	0.207	0.219	0.197	0.210
Number of id	3,668	3,668	2,685	2,685

This table presents estimates from panel regressions explaining unemployment change where the liquidity injection originates for the period 2010 to 2014. The dependent variable is unemployment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.2. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted period dummy is 2010-2011. $\lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

Table 3.8 *Effect on Δu (Destination).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Matched	(4) Matched Flex
$\lambda^{\geq 2013}$	-0.043			
	(0.053)			
λ^{2012}		0.053		
		(0.122)		
λ^{2013}		-0.142		
		(0.097)		
λ^{2014}		0.049		
		(0.085)		
$\lambda_H^{\geq 2013}$			-0.138**	
			(0.061)	
λ_H^{2012}				0.020
				(0.118)
λ_H^{2013}				-0.171**
				(0.085)
λ_H^{2014}				-0.109
				(0.090)
Observations	13,087	16,359	12,824	16,030
R-squared	0.307	0.327	0.302	0.312
Number of id	3,272	3,272	2,453	2,453

This table presents estimates from panel regressions explaining unemployment change in the municipality where the firms that receive the liquidity injection are located for the period 2010 to 2014. The dependent variable is unemployment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.2. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted period dummy is 2010-2011. $\lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

3.8.4 Results for Employment (Fixed Effects, $\lambda^{2010} = \lambda^{2011}$)

Table 3.9 *Effect on* Δe *(Origin).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Matched	(4) Matched Flex
$\lambda^{\geq 2013}$	0.102			
	(0.093)			
λ^{2012}		0.081		
		(0.099)		
λ^{2013}		0.142		
		(0.107)		
λ^{2014}		0.071		
		(0.111)		
$\lambda_H^{\geq 2013}$			0.208	
			(0.173)	
λ_H^{2012}				0.356
				(0.250)
λ_H^{2013}				0.391*
				(0.218)
λ_H^{2014}				0.034
				(0.198)
Observations	14,605	18,257	13,915	17,401
R-squared	0.053	0.079	0.060	0.085
Number of id	3,660	3,661	2,680	2,681

This table presents estimates from panel regressions explaining employment change where the liquidity injection originates for the period 2010 to 2014. The dependent variable is employment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.2. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted period dummy is 2010-2011. $\lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

Table 3.10 *Effect on Δe (Destination).*

VARIABLES	(1) Benchmark	(2) Flexible	(3) Matched	(4) Matched Flex
$\lambda^{\geq 2013}$	0.511**			
	(0.242)			
λ^{2012}		-0.210		
		(0.236)		
λ^{2013}		0.481**		
		(0.200)		
λ^{2014}		0.542*		
		(0.325)		
$\lambda_H^{\geq 2013}$			0.460**	
			(0.199)	
λ_H^{2012}				0.263
				(0.256)
λ_H^{2013}				0.421*
				(0.238)
λ_H^{2014}				0.499**
				(0.217)
Observations	13,065	16,330	12,821	16,026
R-squared	0.074	0.101	0.084	0.116
Number of id	3,270	3,270	2,453	2,453

This table presents estimates from panel regressions explaining employment change in the municipality where the firms that receive the liquidity injection are located for the period 2010 to 2014. The dependent variable is employment and measured as indicated in Section 3.5.2.1. All columns show results following the specification explained in Section 3.6.2. Column 1 shows results following the specification detailed in Section 3.5.2.1 and $\lambda^{\geq 2013}$ is the coefficient that interacts the liquidity shock and a dummy variable that takes value 1 for the period 2013-14. Column 2 reports results for the flexible specification explained in Section 3.5.2.2 in which the omitted period dummy is 2010-2011. $\lambda^{2012}, \lambda^{2013}, \lambda^{2014}$ report coefficients for the interactions of the liquidity shock and yearly dummies in 2012, 2013 and 2014 respectively. Column 3 reports results following the specification detailed in Section 3.5.2.3 in which $\lambda_H^{\geq 2013}$ is the coefficient that interacts the binary liquidity shock dummy and a dummy variable for the period 2013-14. Column 4 reports results for the binary specification explained in Section 3.5.2.3 where λ_H^{2012}, λ_H^{2013} and λ_H^{2014} are the coefficients that interact the binary liquidity shock dummy and dummies for the years 2012, 2013 and 2014 respectively. Columns 5 and 6 follow the specification explained in Section 3.5.2.4. The difference with results of columns 3 and 4 is that in columns 5 and 6 the matched sample is used and therefore there are less observations.

3.8.5 Quality of the matching procedure

Table 3.11 *Quality of the match in 2010 (Origin)*

	Low (U)	High (U)	Low (M)	High (M)
X coordinate	0.46	0.52	0.54	0.52
	(0.32)	(0.28)	(0.33)	(0.28)
Y coordinate	4.43	4.42	4.41	4.42
	(0.28)	(0.24)	(0.27)	(0.24)
Population	10054.49	9186.97	10842.75	9333.35
	(36420.56)	(83762.62)	(38656.84)	(84563.89)
Unemployment rate	0.10	0.10	0.10	0.10
	(0.04)	(0.04)	(0.04)	(0.04)
Employment rate	0.38	0.37	0.39	0.37
	(0.17)	(0.18)	(0.16)	(0.18)
Fraction of population aged 15-64	0.69	0.69	0.70	0.69
	(0.07)	(0.07)	(0.07)	(0.07)
Percentage PSOE	0.35	0.35	0.36	0.35
	(0.18)	(0.19)	(0.18)	(0.19)
Percentage PP	0.38	0.37	0.35	0.37
	(0.21)	(0.22)	(0.21)	(0.22)
Percentage IU	0.05	0.04	0.04	0.04
	(0.10)	(0.10)	(0.09)	(0.10)
Per-capita IAE (EUR/N)	24.85	30.44	30.67	27.42
	(76.46)	(233.33)	(90.91)	(212.54)
Per-capita debt (EUR/N)	386.99	581.12	538.24	546.21
	(458.65)	(621.33)	(534.77)	(545.14)
Per-capita revenue (EUR/N)	1870.70	2249.49	2164.13	2191.76
	(1176.63)	(1478.85)	(1447.96)	(1350.39)
Per-capita expenditure (EUR/N)	1848.50	2229.28	2135.79	2177.64
	(1143.76)	(1409.99)	(1405.68)	(1309.01)
Observations	1892	1784	936	1750

This table reports summary statistics of municipality-year observations in 2010. The data is classified according to the origin of the funds, this is, according to the municipality that owed money to the firm. Municipalities are classified as "Low" (column 1 and 3) if the money received is below the median per-capita liquidity received by all municipalities and they are classified as "High" (column 2 and 4) otherwise. Columns 1 and 2 present raw results before the matching is implemented. Columns 3 and 4 present results after the matching is realized. X and Y coordinate geographically locate each municipality, population is the amount of people registered in the census of the municipality, unemployment rate is the ratio of unemployed population to working age population, employment rate is the ratio of employed population to working age population, fraction of population aged 15-64 is the percentage of population aged 15-64, percentage PSOE, percentage PP and percentage IU are the percentage of total votes received by PSOE, PP and IU in the 2011 municipal elections, per-capita IAE, per-capita debt, per-capita revenue and per-capita expenditure measure the municipal revenue from the IAE (tax on economic activities), the municipal debt, total municipal revenues and total municipal expenditures normalized by working age population. Numbers reported are cross-sectional averages and standard errors in parentheses. Number of observations appear in the last row.

Table 3.12 *Quality of the match in 2011 (Origin)*

	Low (U)	High (U)	Low (M)	High (M)
X coordinate	0.46	0.52	0.54	0.52
	(0.32)	(0.28)	(0.33)	(0.28)
Y coordinate	4.43	4.42	4.41	4.42
	(0.28)	(0.24)	(0.27)	(0.24)
Population	10085.54	9218.34	10891.67	9364.78
	(36406.21)	(83546.39)	(38663.79)	(84345.44)
Unemployment rate	0.12	0.12	0.12	0.12
	(0.05)	(0.05)	(0.05)	(0.05)
Employment rate	0.40	0.39	0.41	0.39
	(0.18)	(0.19)	(0.17)	(0.19)
Fraction of population aged 15-64	0.64	0.63	0.64	0.63
	(0.06)	(0.06)	(0.06)	(0.06)
Percentage PSOE	0.35	0.35	0.36	0.35
	(0.18)	(0.19)	(0.18)	(0.19)
Percentage PP	0.38	0.37	0.35	0.37
	(0.21)	(0.22)	(0.21)	(0.22)
Percentage IU	0.05	0.04	0.04	0.04
	(0.10)	(0.10)	(0.09)	(0.10)
Per-capita IAE (EUR/N)	28.09	34.28	33.89	31.24
	(89.90)	(263.78)	(102.59)	(245.31)
Per-capita debt (EUR/N)	414.79	622.12	568.97	587.81
	(471.29)	(645.67)	(537.78)	(571.68)
Per-capita revenue (EUR/N)	1678.11	2022.59	1827.10	1989.77
	(924.00)	(1277.59)	(961.62)	(1236.53)
Per-capita expenditure (EUR/N)	1701.21	2068.49	1858.45	2028.51
	(936.25)	(1292.66)	(994.25)	(1233.49)
Observations	1893	1784	936	1750

This table reports summary statistics of municipality-year observations in 2011 for the entire sample. The data is classified according to the origin of the funds, this is, according to the municipality that owed money to the firm. Municipalities are classified as "Low" (column 1 and 3) if the money received is below the median per-capita liquidity received by all municipalities and they are classified as "High" (column 2 and 4) otherwise. Columns 1 and 2 present raw results before the matching is implemented. Columns 3 and 4 present results after the matching is realized. X coordinate and Y coordinate geographically locate each municipality and represent the centroid of the 2-dimensional coordinates of each municipality on a map in geospatial vector data format, population is the amount of people registered in the census of the municipality, unemployment rate is the ratio of unemployed population to working age population, employment rate is the ratio of employed population to working age population, fraction of population aged 15-64 is the percentage of population aged 15-64, percentage PSOE, percentage PP and percentage IU are the percentage of total votes received by PSOE, PP and IU in the 2011 municipal elections, per-capita IAE, per-capita debt, per-capita revenue and per-capita expenditure measure the municipal revenue from the IAE (tax on economic activities), the municipal debt, total municipal revenues and total municipal expenditures normalized by working age population. Numbers reported are cross-sectional averages and standard errors in parentheses. Number of observations appear in the last row.

Table 3.13 *Quality of the match in 2010 (Destination)*

	Low (U)	High (U)	Low (M)	High (M)
X coordinate	0.48	0.48	0.49	0.48
	(0.34)	(0.31)	(0.39)	(0.31)
Y coordinate	4.47	4.41	4.41	4.41
	(0.28)	(0.28)	(0.33)	(0.28)
Population	7585.60	18128.60	11653.83	10142.93
	(18933.18)	(101587.07)	(25638.01)	(21029.69)
Unemployment rate	0.10	0.11	0.11	0.11
	(0.04)	(0.04)	(0.04)	(0.04)
Employment rate	0.40	0.44	0.42	0.42
	(0.22)	(0.28)	(0.19)	(0.20)
Fraction of population aged 15-64	0.71	0.71	0.71	0.71
	(0.06)	(0.06)	(0.06)	(0.06)
Percentage PSOE	0.33	0.34	0.33	0.34
	(0.18)	(0.18)	(0.17)	(0.18)
Percentage PP	0.35	0.37	0.35	0.37
	(0.22)	(0.20)	(0.22)	(0.20)
Percentage IU	0.04	0.05	0.04	0.05
	(0.09)	(0.10)	(0.09)	(0.10)
Per-capita IAE (EUR/N)	34.61	39.82	28.40	30.21
	(138.11)	(359.90)	(53.88)	(93.19)
Per-capita debt (EUR/N)	429.78	501.32	436.17	490.19
	(469.10)	(605.67)	(435.17)	(598.74)
Per-capita revenue (EUR/N)	1791.45	1861.00	1826.57	1859.84
	(906.36)	(1141.90)	(1021.59)	(1143.65)
Per-capita expenditure (EUR/N)	1759.01	1853.52	1783.38	1853.84
	(865.34)	(1101.19)	(990.05)	(1105.94)
Observations	1703	1676	851	1605

This table reports summary statistics of municipality-year observations in 2010 for the entire sample. The data is classified according to the destination of the funds, this is, according to the municipality where the firms that receive the money have their headquarters. Municipalities are classified as "Low" (column 1 and 3) if the money received is below the median per-capita liquidity received by all municipalities and they are classified as "High" (column 2 and 4) otherwise. Columns 1 and 2 present raw results before the matching is implemented. Columns 3 and 4 present results after the matching is realized. X coordinate and Y coordinate geographically locate each municipality and represent the centroid of the 2-dimensional coordinates of each municipality on a map in geospatial vector data format, population is the amount of people registered in the census of the municipality, unemployment rate is the ratio of unemployed population to working age population, employment rate is the ratio of employed population to working age population, fraction of population aged 15-64 is the percentage of population aged 15-64, percentage PSOE, percentage PP and percentage IU are the percentage of total votes received by PSOE, PP and IU in the 2011 municipal elections, per-capita IAE, per-capita debt, per-capita revenue and per-capita expenditure measure the municipal revenue from the IAE (tax on economic activities), the municipal debt, total municipal revenues and total municipal expenditures normalized by working age population. Numbers reported are cross-sectional averages and standard errors in parentheses. Number of observations appear in the last row.

Table 3.14 *Quality of the match in 2011 (Destination)*

	Low (U)	High (U)	Low (M)	High (M)
X coordinate	0.48	0.48	0.49	0.48
	(0.34)	(0.31)	(0.39)	(0.31)
Y coordinate	4.47	4.41	4.41	4.41
	(0.28)	(0.28)	(0.33)	(0.28)
Population	7618.61	18179.21	11722.78	10204.00
	(18973.31)	(101352.04)	(25756.65)	(21109.32)
Unemployment rate	0.12	0.12	0.12	0.12
	(0.04)	(0.05)	(0.04)	(0.05)
Employment rate	0.42	0.46	0.44	0.44
	(0.24)	(0.29)	(0.21)	(0.21)
Fraction of population aged 15-64	0.65	0.65	0.65	0.65
	(0.05)	(0.05)	(0.05)	(0.05)
Percentage PSOE	0.33	0.34	0.33	0.34
	(0.18)	(0.18)	(0.17)	(0.18)
Percentage PP	0.35	0.37	0.35	0.37
	(0.22)	(0.20)	(0.22)	(0.20)
Percentage IU	0.04	0.05	0.04	0.05
	(0.09)	(0.10)	(0.09)	(0.10)
Per-capita IAE (EUR/N)	39.53	46.31	33.46	36.36
	(148.81)	(394.28)	(81.33)	(152.50)
Per-capita debt (EUR/N)	458.42	537.39	466.15	526.67
	(504.28)	(628.61)	(462.50)	(622.23)
Per-capita revenue (EUR/N)	1635.11	1687.43	1655.26	1686.30
	(796.59)	(968.81)	(782.98)	(972.89)
Per-capita expenditure (EUR/N)	1640.15	1701.24	1660.46	1696.27
	(775.39)	(937.32)	(751.41)	(933.42)
Observations	1703	1676	851	1605

This table reports summary statistics of municipality-year observations in 2011 for the entire sample. The data is classified according to the destination of the funds, this is, according to the municipality where the firms that receive the money have their headquarters. Municipalities are classified as "Low" (column 1 and 3) if the money received is below the median per-capita liquidity received by all municipalities and they are classified as "High" (column 2 and 4) otherwise. Columns 1 and 2 present raw results before the matching is implemented. Columns 3 and 4 present results after the matching is realized. X coordinate and Y coordinate geographically locate each municipality and represent the centroid of the 2-dimensional coordinates of each municipality on a map in geospatial vector data format, population is the amount of people registered in the census of the municipality, unemployment rate is the ratio of unemployed population to working age population, employment rate is the ratio of employed population to working age population, fraction of population aged 15-64 is the percentage of population aged 15-64, percentage PSOE, percentage PP and percentage IU are the percentage of total votes received by PSOE, PP and IU in the 2011 municipal elections, per-capita IAE, per-capita debt, per-capita revenue and per-capita expenditure measure the municipal revenue from the IAE (tax on economic activities), the municipal debt, total municipal revenues and total municipal expenditures normalized by working age population. Numbers reported are cross-sectional averages and standard errors in parentheses. Number of observations appear in the last row.

4 Do Cross-Listings substitute for International Diversification?

4.1 Introduction

In the last three decades international equity positions in portfolio investment have increased substantially. The demand from investors to increase the international exposure of their portfolio has increased. At the same time, firms have opted to increase their cross-listing in foreign markets as a way to broaden their investor base. These changes in the relative demand and supply for cross-border investments raise questions on the relative value from international portfolio diversification. What have the effects of these two trends been on the net benefits from international portfolio diversification? How have the relative values in terms of industry and country diversification changed as a result of cross-listing by corporations?

This paper focuses on these questions which are of paramount importance for optimal portfolio investment strategies. First, it provides a description of the evolution of cross-listing in international financial markets over the last three decades. The propensity to cross-list has increased over the last decades. Whereas in the 1980s there were roughly 1,000 cross-listed firms, this figure rose to almost 11,000 in the 2000s. In relative terms, cross-listed stocks, which represented less than 7% of total listed stocks in the 1980s, accounted for over 17% of total listed stocks in the 2000s.

Second, our paper shows that the presence of cross-listings significantly changes the relative gains from international portfolio diversification. There has been a long literature examining the importance of industry and country sources of diversification when looking at international portfolio allocation. On one end, this literature has highlighted the prevalence of country shocks relative to industry shocks in internationally diversified portfolios (e.g., Heston and Rouwenhorst (1994) and Griffin and Karolyi (1998)). This prevalence implied that there were substantial benefits from diversification across countries in a portfolio. On a different end of the spectrum, a number of papers have highlighted that the gains from international diversification for a US based investor could be essentially achieved by investing in available securities traded in U.S. markets. For example, in a classical paper, Errunza et al. (1999) show that a diversified portfolio of U.S. issued securities that included foreign firms listed in

U.S. markets, international close-end funds, and U.S. multinationals essentially could replicate a portfolio of international diversified securities.

This paper provides additional insights to integrate these different views on the benefits from international portfolio diversification. The paper shows that the prevalence of cross-listed firms has implied a shift in the relative role that industry and country shocks play in an internationally diversified portfolio. We are the first to explore the role of cross-listed securities in the literature on country and industry effects. By using a more complete data set, we show that an internationally diversified portfolio with the inclusion of cross-listed securities resulted in a significant decline in the size of country shocks in the last decade as country shocks became less important than industry shocks. The paper shows that this effect is mainly driven by the presence of cross-listed firms. In an internationally diversified portfolio that does not include investment in cross-listed securities, diversification across countries is more effective than across industries. Including cross-listed securities lowers the importance of country effects and is therefore an effective way to achieve international diversification.

Cross-listing may possibly be the indicator for many other factors, for example size, the dominance of multinational business, etc. A complete characterization of all possible driving forces in the international diversification process would require a different empirical strategy in order to disentangle country effects from these underlying forces. However, from the point of view of an investor who must decide whether to diversify across countries or industries, these underlying reasons are irrelevant and we therefore do not study them.

The rest of the paper is organized as follows. The next section presents a short literature review on the benefits from international portfolio diversification and the sources for this diversification benefits identified in the literature. Section 3 describes the data and provides evidence on the evolution of cross-listing among international corporations, as well as the main determinants in identifying cross-listing patterns. Section 4 provides the methodological approach to identify relative gains from international portfolio diversification. Section 5 presents the empirical evidence on the role of cross-listing firms in the sources of gains from international diversification and Section 6 concludes.

4.2 Literature Review

After the intrinsic tradeoff between risk and return was formally settled by Markowitz (1952), ways of reducing risk through diversification were extensively studied. Potential gains from industrial and international portfolio diversification have been measured in a large literature started by Grubel (1968) and Levy and Sarnat (1970). Heston and Rouwenhorst (1994) showed that diversification across countries within an industry is a much more effective tool for risk reduction than industry diversification within a country even in geographically concentrated and economically integrated regions such as Western Europe. In this same line, Griffin and Karolyi (1998) confirmed this result for a broader sample of countries that included emerging markets.

Following Rouwenhorst (1999), a series of researchers including Baca et al. (2000), Ferreira et al. (2005), Campa and Fernandes (2006) and Faias and Ferreira (2015) have analyzed the evolution of country and industry factors and have concluded that industry factors have been growing in importance and may now dominate country factors. However, Brooks and Del Negro (2004) and Soriano and Climent (2006) argue that this may have been just a temporary phenomenon associated with the stock market bubble at the beginning of the century.

More recently, a number of authors have used different methodologies with mixed results on the relative importance of country and industry factors. Thus, the debate is still open. Phylaktis and Xia (2006), Baele and Inghelbrecht (2009) and Bekaert et al. (2009) have found that geographical diversification continues to be superior to industry diversification, while Bai and Green (2010) and Eiling et al. (2012) stated that international equity returns are mainly driven by industry factors. Moreover, Chen et al. (2006) and Christoffersen et al. (2012) have argued that there has been a decline of the benefits from international diversification over time, and showed that emerging markets offer greater diversification benefits than developed markets. A wide variety of adaptations to the methodology of Heston and Rouwenhorst (1994) and alternative models have been suggested. Because the model by Heston and Rouwenhorst (1994) is widely used by practitioners, and also the conventional workhorse of the analysis of country and industry effects, we restrict our analysis to their methodology.

Another strand of the literature has argued that investors can effectively diversify across countries with instruments available in their home markets. Errunza et al. (1999) tested this hypothesis and found that home-made diversification through investment in multinational corporation stocks, closed-end country funds, and American Depositary Receipts essentially exhausts gains from international diversification. Moreover, they claim that incremental gains from international diversification beyond home-made diversification portfolios have diminished over time in a way consistent with changes in investment barriers. Rowland and Tesar (2004) also showed that multinational corporations, which are usually included in the literature on industry vs country effects, have provided diversification benefits for investors.

A number of recent articles have argued that home bias and investor's preferences for home-stocks may limit the effectiveness of the diversification benefits highlighted by Errunza et al. (1999). Portes and Rey (2005) showed that investors prefer the stocks of foreign countries that are closer and Chan et al. (2005) showed that investors prefer the stocks of foreign countries whose equity markets are more, not less, correlated with their own. Moreover, the decision of where to cross-list may itself limit the effectiveness of international diversification. Sarkissian and Schill (2004) argued that geographic, cultural, and industrial proximity are the main determinants of choosing where to cross list. They found that contrary to the notion that firms maximize international portfolio diversification gains in listing abroad, cross-listing activity is more common across markets for which diversification gains are relatively low. The main finding in this paper, that adding cross-listed firms reduces international portfolio diversification gains, contrasts with this view.

4.3 The Phenomenon of Cross-Listing

Equity cross-listing by firms has increased over time. The prevalence of cross listing differs by country of origin and firms are most likely to cross-list in relatively large financial markets close to their home base. To grasp the full extent of the cross-listing phenomenon, and to consider the whole universe of alternative stocks available to an investor: we evaluate the largest possible set of information. We focus our analysis on the information available in Global Index data. This database is used by virtually all of the academic literature. This provides the added benefit of comparability relative to previous work. We use a dataset covering the entire menu of alternative stocks in which an investor can invest.[1]

We focus our analysis on firm level data and include all stocks listed in a particular country. The use of firm level data allows the inclusion of firms with relative small market capitalization that are usually excluded from the reported country and industry indexes that have been usually used in the previous literature.

Bai and Green (2010) argue that the use of data on individual stocks is preferable to using indices because indices have several limitations: first, investment managers buy individual shares and not indices, and second, weighting and composition of indices change over time in a manner that does not necessarily reflect underlying market trends. We follow their lead and use individual stocks as our unit of analysis.

All data are monthly and in US dollars, expressed at the current exchange rate. The return data that we use are monthly total return indices and market capitalizations for each firm. Total return indices represent the theoretical growth in value of holding a stock over a month, assuming that dividends and other payments are re-invested to purchase additional units of equity and correcting for stock splits. The dataset used in this paper covers stocks traded in 42 countries, 22 developed and 20 emerging markets, over three decades (from December 1979 to December 2009). Stocks traded in these 42 national markets can belong to either domestic firms based in each of these countries

[1] The data comes from Datastream. We use research lists. These lists include all stocks listed in a particular country and do not a priori exclude stocks based on certain criteria, such as market capitalization.

or to foreign firms cross-listed in those markets. The origin of cross-listed firms may be from countries other than the 42 that we consider. In total, in our data set we have identified 97 different countries of origin of the cross-listed firms.

Table 1 shows the prevalence of cross-listed firms in each country and the prevalence of these observations over time. The first column lists the 42 countries in our data. The following six columns show the number of firms in each country that are cross-listed and the total of listed firms by period. Two salient features arise from these data. First, cross-listing has become more important over time. Both the number and the percentage of cross-listed firms have been increasing. In the 1980s there were 1,026 cross-listed firms (6.7% of the total) and by the 2000s this number had risen to 10,994 (17.2% of the total). Second, cross-listing is much more concentrated in a few countries. Large developed countries (US, Great Britain, Germany) and small countries with large financial centers (Hong Kong, Singapore) account for the vast majority of cross-listings in the sample. However, the concentration of cross listings has not changed monotonically over time. For example, the C3 concentration ratio stood at 64% in the 1980s, dropped to 52% in the 1990s, and increased to 72% in the 2000s.[2] For the C5 concentration ratio, the numbers were 85% in the 1980s, 72% in the 1990s, and 86% in the 2000s. In the 2000s, Germany becomes the second largest destination for cross-listings. Switzerland also attracts a greater number of cross-listed stocks. On the other hand, cross-listings in France and Singapore do not grow as much.

The last six columns list the number of observations in our database. An observation in these columns is a firm-month pair. Cross-listing has become more stable over time. In the 1980s there were countries with a relative large number of cross-listed firms that did not remain in the sample for very long. The persistence of cross-listed firms in the latter part of the sample is larger. Therefore, the correlation coefficient between the number of firms and the number of observations rises from around 0.92 in the 1980s and 1990s to over 0.96 in the 2000s.[3]

Table 2 shows the origin of firms cross-listed in our 42 markets. We have grouped the 97 originating countries that appear in the data into regions. Most of the cross-listed firms originate from the Americas and Europe. Within those continents, North America and Western and Northern Europe have the largest share of firms. The Caribbean is over-represented, presumably because of tax benefits.

We classify firms according to their primary activity. We use Datastream Industrial Classification Levels, which is analogous to the Industry Classification Benchmark developed by FTSE.[4] We group firms according to their industry

[2] Concentration ratio C"n" is calculated by adding the number of cross-listed firms in the "n" countries with more cross-listed firms and dividing by the total number of cross-listed firms.

[3] This correlation coefficient is calculated per period, using the number of cross-listed firms and the number of cross-listed observations as the two arrays.

[4] All firms that do not have an industrial classification are dropped. Moreover, those firms that belong to the "Unquoted Equity Classification" are also dropped.

classification into 35 groups, so that the number of industries roughly coincides with the number of countries. Griffin and Karolyi (1998) argue that the number of countries and industries must be similar to more accurately measure the country and industry effects.[5]

In the first column of Table 3 we observe the 35 industries used in the paper. The next 3 columns show the ratio of total market value of cross-listings over total market value of listed companies (both cross-listings and non-cross-listings). The last six columns present the number of firms that are cross-listed and the total number of firms (both cross-listed and non-cross-listed) by industry, for the three decades considered.

In the last row of Table 3 we observe that cross-listings are not only increasing in number (right of the table) but also their relative market value weight is also increasing (left of the table). This is evidence that the propensity to cross-list has increased not only in terms of the number of firms, but also in their relative weight with respect to total listed firms. As we observe, this increase is practically homogeneous among all industries, although some industries, such as general industrials, have permanently had a large weight of cross-listings. The large fraction of market value accounted for by cross-listed firms implies that investors can easily invest in each industry, and potentially diversify across industries, using cross-listed firms.

In Table 4 we look at mean returns, standard deviations and Sharpe ratios for country portfolios of value weighted local stocks and cross-listed firms. If cross-listing provides diversification benefits, then equilibrium expected returns could be lower for cross-listed firms. In fact, we observe that all countries with a high number of cross-listings (see Table 1), such as USA, Great Britain, Germany, Hong Kong or Singapore, have a lower Sharpe Ratio for their cross-listings than for the non-cross-listings. Therefore, the evidence is broadly consistent with the existence of a diversification premium.

4.4 Methodology

We have characterized the nature of the cross-listing phenomenon. Now we move on to study whether cross-listings have an impact on diversification possibilities. We do so in the context of the ubiquitous Heston and Rouwenhorst (1994) framework. In this framework it is assumed that returns depend on a global market factor and industry and country factors. Specifically, the return of the ith security that belongs to industry j and country k can be decomposed as:

[5] As we explain below, although the total number of countries is 42, not all countries have data at each date, so that on average the effective number of countries is close to 35 in our regressions.

$$R_{ijk}(t) = \alpha(t) + \beta_j(t) + \gamma_k(t) + \varepsilon_{ijk}(t) \tag{4.1}$$

where $R_{ijk}(t)$ is the total return index of firm i that belongs to industry j and country k in month t, $\alpha(t)$ is the base level return in period t, $\beta_j(t)$ is the industry factor in month t, $\gamma_k(t)$ is the country factor in month t and $\varepsilon_{ijk}(t)$ represents idiosyncratic unobserved heterogeneity.

For each month t, we estimate the common factor $\alpha(t)$, the industry factor $\beta_j(t)$, and the country factor $\gamma_k(t)$ using a cross-sectional regression of all firms on country and industry dummies:

$$R_{ijk} = \alpha + \beta_1 I_1 + \beta_2 I_2 + ... + \beta_{35} I_{35} + \gamma_1 C_1 + \gamma_2 C_2 + ... + \gamma_{42} C_{42} + \varepsilon_{ijk} \tag{4.2}$$

Again, R_{ijk} is the total return index of firm i that belongs to industry j and country k, β_j and γ_k are the industry and country pure effects, I and C are the industry and country dummies which take value one if firm i belongs to that industry and country, or take value zero otherwise. All cross sectional regressions are estimated through weighted least squares.

As is well known, when using dummy variables as regressors it is not possible to identify the effects of the dummies (industry and country in our case) if all the dummies in each category (one dummy for each industry and one dummy for each country) are included, because of perfect multicollinearity between the regressors. There are several ways of dealing with this issue. One practice is to exclude one industry and one country in the regression. The estimated coefficients are then interpreted as the industry and country effects relative to the industry and country excluded. The solution favored in the literature (Campa and Fernandes (2006); Bekaert et al. (2009)) on country and industry effects is to add two additional restrictions, one for industries and one for countries, to remove the redundant degrees of freedom. This is, in fact, the procedure we will follow in this paper; we add the following two linear constraints:$\sum_{j=1}^{35} \omega_{j,t}^i \beta_{j,t} = 0$ and $\sum_{k=1}^{42} \omega_{k,t}^i \gamma_{k,t} = 0$, where $\omega_{j,t}^i$ and $\omega_{k,t}^i$ are the weights of industry j, and country k, in the world market portfolio at month t. In this way, the weighted sum of the pure industry and country effects add up to zero, and the intercept α is interpreted as the return on the value-weighted world market return at t. A country pure effect γ_k is the excess return of a portfolio of country k that is free of incremental industrial effects. Likewise, an industry pure effect β_j is the excess return of a portfolio of industry j that is free of incremental country effects.

4.4.1 MADs

For each period t, we obtain estimated coefficients for the industry and country effects from the cross-sectional regressions previously described. In order to compare industry and country effects, we use the monthly time series of the coefficients obtained and follow Rouwenhorst (1999) to construct mean absolute deviation (MAD) measures: $MAD_{\beta}(t) = \sum_{j=1}^{35} \omega_j |\beta_t^j|$ and $MAD_{\gamma}(t) = \sum_{k=1}^{42} \omega_k |\gamma_t^k|$ where ω_j and ω_k are the industry and country weights respectively, and $|\beta_t^j|$ and $|\gamma_t^k|$ are the absolute industry and country effects in month t. The $MAD_{\beta}(t)$ measures the weighted mean absolute deviation industry effects, and the $MAD_{\gamma}(t)|$ measures the weighted mean absolute deviation country effects. This measure gauges the importance of the pure industry and country effects and serves as a dispersion measure. The higher the MADs, the higher the dispersion of the weighted absolute estimated coefficients, and thus the more disperse are the industry/country returns around the world in that period. For all our figures, we plot 23-month moving averages of the monthly weighted absolute values of the industry and country effects.

4.4.2 Sample selection

In our analysis, the number of countries does not remain constant through time. We have tried to replicate the universe of investment possibilities available to the typical investor. As new countries appear, international investors are able to diversify over a larger set of countries. Nevertheless, for a given period, if there are less than 35 firms listed in a country, these firms are dropped from the regressions, and therefore the country will not be part of the analysis. This adjustment is not necessary in the industry sectors because the industrial classification levels have been adjusted to have enough firms per level. The reason for excluding countries with a low number of observations is twofold. First, a minimum number of observations is necessary to accurately identify the coefficient econometrically. Second, from the perspective of an investor, the number of firms in a market is negatively correlated with the ability and easiness to invest in frontier markets. In any case, value weighting will reduce the importance of this issue. At the same time, country MADs will pick up country effects for these time-changing investment possibilities. We have conducted a number of robustness exercises in which we restrict the number of countries to those that were available at given moments of time.[6]

[6] Results are available upon request.

4.5 Results

4.5.1 The relative importance of industry and country effects

We use the same empirical approach throughout all this section but we gradually vary our sample by including cross-listings. In our first approach, following what has been done in the literature, we omit cross-listings from our sample, both in the regressions and in the calculation of the MADs. At each date we use all available countries. When observations of cross-listed firms are excluded from the sample, country factors dominate industry factors, except for the period of the stock market bubble. In Figure 1 we plot 23-month moving averages of industry and country MADs. As documented by Brooks and Del Negro (2004) and Soriano and Climent (2006), industry MADs temporarily exceed country MADs around the year 2000 due to the stock market dot-com bubble and crash. Consistent with their findings, in our data, it is the industries of software and computer services, technology hardware and equipment, and telecommunications that exhibit the largest coefficients during this period. More generally, the picture qualitatively resembles the findings of the previous literature, indicating that our sample and methodology are overall comparable. In the last years of the sample country effects have regained their importance and are above industry effects.

The evidence changes drastically once cross-listed firms are included in the sample. The relative importance of pure country effects is reduced. In Figure 2 country MADs diminish whereas industry MADs are barely affected. In the last years of our sample, industry and country effects are roughly equalized. Because countries that are late entrants into the sample are emerging markets, country effects are given a boost.

A comment on a subtle point is in order. As we have seen in Table IV for markets with a high prevalence of cross-listed firms, cross-listed stocks have a high variance relative to domestic stocks. These countries (US, Great Britain, Germany, Hong Kong, and Singapore) do also represent a large fraction of world market value. Because these markets will carry a large weight in the regressions and construction of MADs, it could be expected that the inclusion of cross-listed firms might mechanically increase country MADs. However, we have found that the inclusion of cross-listed firms reduces country MADs. This implies that the drop in country effects due to increased diversification benefits is particularly strong, given that it overcomes the mechanical effect due to differential variances.

By incorporating cross-listings in the regressions and in the MADs there are basically two distinct phenomena that make country effects decrease. When the weights of the MADs are calculated including cross-listings, more globally integrated countries (that have more cross-listings) increase their weights and consequently country effects decrease. This is because, as we noted before, cross-listings are defined at the country where they are listed, not at the country of origin. The second phenomenon arises by the self-selection of firms that chose to become cross-listed. As shown in Section 3, more globally integrated firms have a higher propensity to cross-list. Therefore, when including cross-listings in the regressions, country effects are likely to decrease. In order to disentangle these two effects, we examine MADs estimated in the sample without cross-listed stocks re-weighted using market values of the sample that includes cross-listings. The results of this exercise are shown in Figure 3. We observe that country effects decrease slightly relative to Figure 1, but still dominate industry effects.

The result from this exercise suggests that the factor driving the reduction of country effects is truly an increase in the benefits of international diversification and that it is not simply due to the rebalancing of global market value to venues in which cross-listed firms are relatively abundant.

4.5.2 Understanding the effect of cross-listings on country effects

What is the effect of cross-listed stocks on country effects? What is the marginal contribution of cross-listed stocks on country effects? To answer this question we use a more flexible specification to break the constraint imposed in equation 1 that cross-listed and purely domestic stocks should command the same country effect. Specifically, the return of a stock in country k can have a different effect depending on whether the stock is cross-listed or not. Whereas the return of a stock that is not cross-listed is similar to equation 1, the return of a cross-listed stock has a country effect $\gamma_k^C L$ that is potentially different from the country effect for domestic stocks γ_k^D. For cross-listed stocks: $R_{ijk}(t) = \alpha(t) + \beta_j^D(t) + \varepsilon_{ijk}(t)$ if it is not cross-listed, and $R_{ijk}(t) = \alpha(t) + \beta_j^{CL}(t) + \varepsilon_{ijk}(t)$ if it is cross-listed.

In terms of the estimation, this amounts to adding interaction terms between country dummies and a dummy indicating whether a particular stock is cross-listed.

$$\begin{aligned} R_{ijk} =& \alpha + \beta_1 I_1 + \beta_2 I_2 + ... + \beta_{35} I_{35} + \gamma_1 C_1 + \gamma_2 C_2 + ... + \gamma_{42} C_{42} \\ &+ \rho_1 C_1 S + \rho_2 C_2 S + ... + \rho_{42} C_{42} S + \varepsilon_{ijk} \end{aligned} \tag{4.3}$$

Again, R_{ijk} is the total return index of firm i that belongs to industry j and country k, β_j, γ_k and ρ_k are the industry, country and cross-listing pure effects, I and C are the industry and country dummies which take value one if firm i belongs to that industry and country, or take value zero otherwise. S is a dummy which takes value one if firm i is cross-listed, or takes value zero otherwise.[7]

The pure country effect attributable to non-cross-listed stocks is directly given by the estimate of γ_k^D. On the other hand, the pure country effect attributable to cross-

[7] To avoid sample size bias, we require that there are at least 4 cross-listed firms in a country in a given period.

listed firms can be calculated as the sum of the domestic country effect and the coefficient on the interaction term: $\gamma_k^{CL} = \gamma_k^D + \rho_k$.

The correlation of γ_k^D with γ_k^{CL} measures the relationship between the country effects attributable to domestic and cross-listed stocks. If this correlation is close to one, then the country effect of domestic and cross-listed stocks moves in lock-step. On the other hand, a decreasing correlation indicates a decoupling of the country effects of domestic and cross-listed stocks. Cross-listed securities become more different from their domestic counterparts. This suggests that they are more useful for diversification. From Figure 4 it is apparent that cross-sectional correlation of γ_k^D and γ_k^{CL} has decreased over time, in particular in the period after the mid-1990s. At the beginning of our sample, domestic and cross-listed stocks had a country effect that tended to move in lock-step whereas, in the latter period, this strong relationship starts to break down.

The wedge between γ_k^D and γ_k^{CL} is given by $\rho_k \equiv \gamma_k^{CL} - \gamma_k^D$. In the data the correlation between the estimates of γ_k^D and the estimates of ρ_k across countries is negative and becomes increasingly negative over time. This indicates that a stock's country effect is counteracted by values of ρ_k that are of the opposite sign in the case of cross-listed firms. In other words, the country effect of cross-listed firms is a muted version of the country effect of domestic stocks, implying that cross-listed stocks are more similar to the world market portfolio (α). This evidence reinforces the conclusion that cross-listed stocks are effective to diversify across countries.

4.6 Conclusion

We provide evidence of the rise of cross-listings in world financial markets. This fact cannot be eluded by investors, because it has an effect on the risk management of equity portfolios. The literature on country and industry effects has not explored the effects of international diversification through home investment through the use of cross-listed stocks. By using a more complete dataset, which includes cross-listings, we find that the relative importance of industry and country effects depends heavily on the inclusion of cross-listings.

Echoing the findings by Errunza et al. (1999), we find that industry effects have become a more effective tool for risk reduction over time relative to country effects. In the 1980s we do not find an important effect of the cross-listings on the country and industry effects relationship, but from the 1990s onward, industry effects gain in importance relative to country effects. This shift in importance is mainly driven by the presence of cross-listed firms since we show that an internationally diversified portfolio that does not include cross-listed securities still has the characteristics that country shocks dominate industry effects.

Most academic research on the importance of industry and country effects has implicitly disregarded cross-listed securities by using databases in which they are not included. Given that they are available to the investment community, the question of how they affect diversification possibilities has practical relevance. So far, the evidence suggests that international diversification is becoming less important than diversification across industries.

4.7 Figures and Tables

Table 4.1 *Summary statistics by country.*

	NUMBER OF FIRMS						NUMBER OF OBSERVATIONS					
	1980-89		1990-99		2000-09		1980-89		1990-99		2000-09	
	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL
Argentina	-	-	0	74	5	102	-	-	0	5.467	481	7.885
Australia	13	378	64	1.265	80	2.545	472	15.609	3.628	76.715	4.225	153.919
Austria	0	35	21	199	23	251	0	2.260	321	15.105	930	15.839
Belgium	39	186	63	353	70	453	1.613	11.256	5.555	28.182	3.455	29.757
Brazil	-	-	0	760	0	783	-	-	0	26.657	0	48.515
Canada	43	1.911	135	4.807	205	6.012	1.287	87.189	6.518	270.742	8.312	311.915
Switzerland	5	243	166	610	431	830	290	17.768	16.425	55.862	27.047	60.570
Chile	0	128	0	256	1	254	0	685	0	20.281	1	20.679
China	-	-	0	998	0	1.846	-	-	0	44.941	0	165.209
Colombia	-	-	0	28	1	107	-	-	0	2.885	1	5.116
Germany	23	360	549	1.276	3.158	4.370	330	15.764	8.612	59.799	99.601	195.620
Denmark	1	189	8	309	14	338	21	7.621	618	28.221	703	23.769
Spain	0	79	13	194	53	283	0	1.917	407	13.937	3.400	20.959
Finland	0	16	2	186	7	238	0	786	79	11.896	358	18.576
France	160	584	256	1.320	259	1.649	1.648	17.861	22.686	96.089	13.138	109.581
United Kingdom	312	1.592	721	3.009	1.369	4.651	22.573	127.955	37.943	219.358	61.013	262.281
Greece	0	102	0	344	2	411	0	2.185	0	24.520	146	36.444
Hong Kong	120	263	526	723	1.143	1.355	4.468	12.393	35.321	55.512	85.870	106.887
Hungary	-	-	0	31	2	76	-	-	0	1.742	59	4.512
Indonesia	-	-	0	315	0	234	-	-	0	16.890	0	7.874
India	-	-	0	2.987	0	4.048	-	-	0	192.756	0	270.570
Ireland	7	36	29	112	28	120	252	2.872	1.299	8.226	970	7.489
Italy	0	259	7	427	52	575	0	15.270	254	35.492	2.268	39.735
Japan	2	1.903	78	3.497	55	4.704	45	123.175	6.742	333.545	3.134	438.301
South Korea	0	637	1	1.353	9	2.030	0	24.000	1	96.309	122	140.349
Luxembourg	-	-	33	51	31	82	-	-	830	4.064	1.213	4.513
Mexico	0	70	53	370	44	288	24	1.286	2.215	19.362	1.033	15.953
Netherlands	12	174	109	433	111	420	893	15.118	3.445	27.393	4.496	24.388
Norway	0	36	28	281	89	448	0	3.972	969	16.410	3.726	25.223
New Zealand	3	43	41	178	53	274	144	1.470	1.514	12.115	2.349	16.438
Philippines	0	52	1	282	2	282	0	72	4	17.898	238	21.343
Poland	-	-	0	148	28	603	-	-	0	4.614	967	29.269
Portugal	0	77	0	154	4	137	0	1.520	0	12.440	119	7.884
Romania	-	-	0	112	1	151	-	-	0	3.191	23	9.210
Russia	-	-	0	226	2	906	-	-	35	3.743	10	23.182
Singapore	98	230	159	521	196	955	7.742	16.989	12.049	38.370	10.634	68.790
Sweden	0	122	15	527	57	845	0	4.920	253	30.473	1.700	51.524
Thailand	0	202	0	565	0	597	0	4.325	0	42.930	0	44.034
Turkey	0	62	1	249	0	311	0	1.236	22	17.931	0	30.254
Taiwan	0	131	0	647	1	998	0	1.690	0	40.738	1	89.263
United States	186	5.247	684	15.102	3.354	17.464	12.105	392.207	33.362	793.006	127.452	857.879
South Africa	2	76	27	842	54	799	240	7.419	2.276	57.555	2.486	45.074
Total	1.026	15.423	3.790	46.121	10.994	63.825	54.147	938.790	203.383	2.883.362	471.681	3.866.572

Notes: This table reports time series averages of the number of listed firms and cross-listed firms and of the number of observations by country for every decade for the period January 1980 to December 2009.

Table 4.2 *Summary statistics by region of origin.*

United Nations REGION	1980-89	1990-99	2000-09
Caribbean	33	163	1.043
Central America	4	10	59
Eastern Africa	1	8	9
Eastern Asia	42	229	612
Eastern Europe	0	5	37
Middle Africa	2	2	2
Northern Africa	0	2	5
Northern America	505	1.912	5.256
Northern Europe	102	370	1.266
Oceania	24	106	609
South America	0	8	66
South-Eastern Asia	110	142	212
Southern Africa	59	87	107
Southern Asia	1	2	0
Southern Europe	15	89	308
Western Africa	1	8	13
Western Asia	21	119	245
Western Europe	106	528	1.145
Total	1.026	3.790	10.994

Notes: This table reports time series averages of the number of cross-listed firms by region of origin for every decade, for the period January 1980 to December 2009.

Table 4.3 *Summary statistics by industry.*

				NUMBER OF FIRMS					
	MV(CL)/MV(TOTAL)			1980-89		1990-99		2000-09	
INDUSTRY	1980-89	1990-99	2000-09	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL	CROSS-LISTED	TOTAL
Aerospace & Defense	15,2%	35,6%	48,1%	8	117	23	239	81	354
Alternative Energy, Oil Equipment, Services & Distribution	52,5%	59,4%	52,5%	25	154	62	538	290	1.056
Automobiles & Parts	26,3%	51,7%	53,2%	22	317	105	915	170	1.078
Banks	21,0%	33,5%	47,9%	60	943	240	2.986	482	3.714
Beverages & Tobacco	33,0%	52,6%	53,7%	17	199	64	559	133	675
Chemicals	18,6%	45,9%	48,8%	26	490	109	1.518	245	1.882
Construction & Materials	4,0%	16,4%	26,8%	32	760	128	1.925	269	2.381
Electricity	7,9%	18,0%	23,0%	12	341	54	837	150	1.375
Electronic & Electrical Equipment	11,0%	21,9%	22,4%	28	792	129	1.935	320	2.349
Financial Services	10,5%	26,1%	29,3%	46	950	157	3.249	646	5.400
Food & Drug Retailers	1,6%	15,2%	37,5%	5	161	25	402	71	506
Food Producers	19,2%	41,0%	36,2%	52	570	112	1.619	270	1.997
Forestry & Paper	6,0%	14,3%	33,5%	11	158	42	415	95	524
Gas, Water & Multiutilities	2,2%	25,4%	52,7%	6	219	36	418	105	550
General Industrials	45,7%	59,5%	65,0%	45	283	113	762	136	761
General Retailers	4,2%	20,3%	41,1%	23	550	79	1.615	289	2.109
Health Care Equipment & Services	20,3%	20,4%	38,3%	12	297	65	1.125	299	1.578
Household Goods & Home Construction	4,5%	40,4%	44,2%	16	442	60	1.067	160	1.198
Industrial Engineering	10,5%	20,4%	23,4%	31	863	107	2.098	260	2.425
Industrial Metals & Mining	13,3%	32,7%	43,1%	33	399	123	1.190	346	1.760
Industrial Transportation	3,3%	19,3%	34,0%	19	350	96	933	268	1.195
Leisure Goods	14,8%	32,5%	26,8%	16	192	67	541	132	716
Life Insurance	12,6%	46,0%	53,0%	10	107	38	248	76	310
Media	12,1%	20,9%	35,7%	18	391	113	1.312	354	2.032
Mining	33,4%	42,4%	41,3%	128	978	285	2.380	1.180	4.082
Nonlife Insurance	5,7%	42,7%	65,6%	15	269	67	609	164	644
Oil & Gas Producers	21,9%	47,0%	47,4%	65	577	181	1.671	631	2.557
Personal Goods	11,9%	22,5%	44,6%	21	555	89	1.631	256	1.836
Pharmaceuticals & Biothecnology	14,5%	44,4%	50,5%	28	299	130	1.277	550	2.206
Real Estate Investment & Services	6,8%	31,8%	26,6%	59	567	149	1.706	352	2.153
Software & Computer Services	4,7%	34,8%	47,6%	18	365	192	2.446	695	4.076
Support Services	15,2%	29,3%	30,8%	29	630	114	1.853	347	2.441
Technology Hardware & Equipment	17,9%	34,7%	54,6%	26	370	177	1.408	531	2.443
Telecommunications	18,1%	42,6%	51,6%	21	176	117	911	268	1.257
Travel & Leisure	8,5%	25,5%	34,9%	43	592	142	1.783	373	2.205
AVERAGE/TOTAL	15,1%	33,3%	41,9%	1.026	15.423	3.790	46.121	10.994	63.825

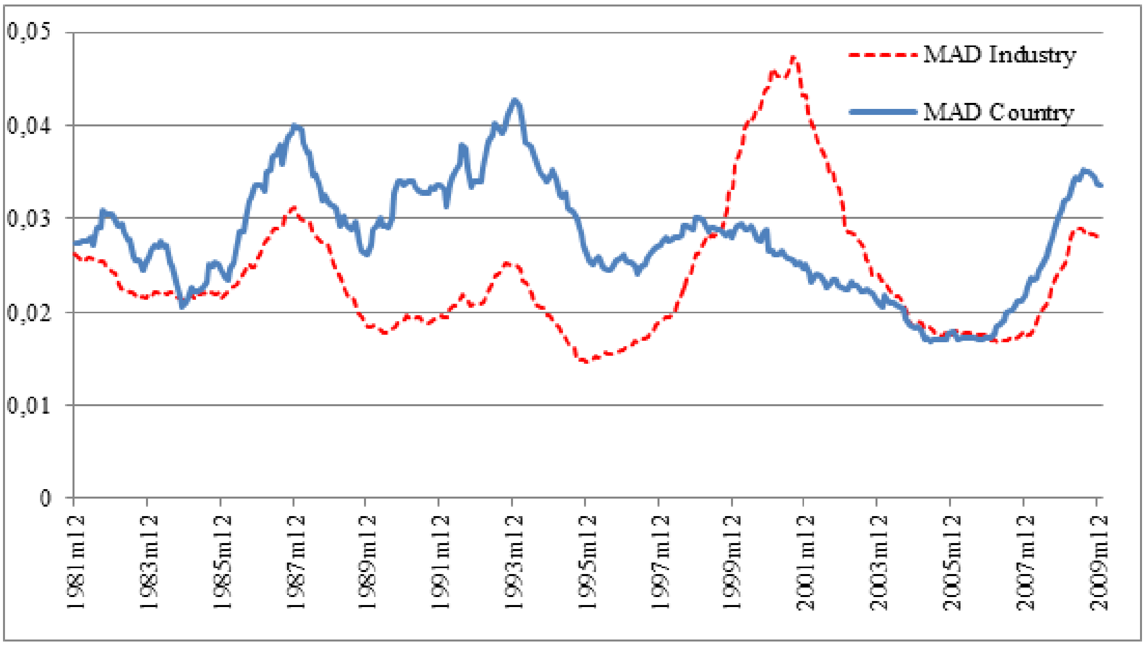
Notes: This table reports time series averages of the market value of cross-listings as a percentage of total market capitalization and of the number of listed firms and cross-listed firms, by industry, for every decade for the period January 1980 to December 2009. The fraction of the market value by cross-listed firms is calculated as MV(CL)/MV(TOTAL) = total market value of cross-listings / (total market value of cross-listings + total market value of non-cross-listings) by industry. Total market value is calculated adding the market value of every firm-month observation. AVERAGE/TOTAL calculates the average for the first three columns and the total for the last six columns.

Table 4.4 *Summary statistics for value weighted portfolios of country returns for local firms and cross-listed firms.*

COUNTRY	NO CROSS-LISTINGS			CROSS-LISTINGS		
	Mean value-weighted return	Value-weighted standard deviation	SHARPE RATIO	Mean value-weighted return	Value-weighted standard deviation	SHARPE RATIO
Argentina	0,98%	10,14%	9,6%	0,76%	5,78%	13,2%
Australia	1,08%	7,60%	14,2%	0,89%	6,70%	13,3%
Austria	0,91%	6,65%	13,7%	0,79%	6,59%	12,0%
Belgium	0,97%	6,58%	14,8%	1,18%	5,70%	20,7%
Brazil	1,17%	14,38%	8,1%	-	-	-
Canada	0,89%	8,96%	10,0%	0,21%	8,52%	2,4%
Switzerland	0,93%	6,15%	15,0%	0,93%	6,65%	14,0%
Chile	1,37%	7,58%	18,1%	8,11%	6,25%	129,7%
China	1,68%	10,36%	16,3%	-	-	-
Colombia	1,54%	8,88%	17,4%	-	-	-
Germany	0,92%	6,71%	13,8%	0,65%	9,41%	6,9%
Denmark	1,21%	7,35%	16,4%	1,33%	5,40%	24,6%
Spain	1,42%	6,89%	20,6%	0,65%	6,44%	10,1%
Finland	1,23%	7,82%	15,8%	1,53%	7,14%	21,4%
France	1,00%	7,42%	13,5%	0,75%	7,35%	10,2%
United Kingdom	0,98%	7,27%	13,5%	0,73%	9,32%	7,8%
Greece	0,94%	10,36%	9,1%	-0,37%	5,84%	-6,4%
Hong Kong	1,42%	7,25%	19,6%	0,78%	8,59%	9,1%
Hungary	1,18%	7,74%	15,3%	-	-	-
Indonesia	1,82%	15,25%	11,9%	-	-	-
India	1,13%	10,58%	10,6%	-	-	-
Ireland	1,06%	8,21%	12,9%	1,24%	5,72%	21,7%
Italy	0,98%	6,96%	14,1%	1,22%	5,96%	20,4%
Japan	0,71%	8,43%	8,4%	0,76%	7,80%	9,8%
South Korea	1,15%	10,95%	10,5%	-0,62%	11,81%	-5,2%
Luxembourg	1,11%	6,93%	16,0%	-	-	-
Mexico	1,30%	8,77%	14,8%	0,64%	7,17%	8,9%
Netherlands	1,06%	6,88%	15,4%	0,89%	5,62%	15,9%
Norway	1,13%	8,29%	13,7%	1,05%	9,06%	11,6%
New Zealand	1,02%	7,42%	13,8%	1,10%	5,28%	20,9%
Philippines	1,16%	10,18%	11,4%	1,12%	3,17%	35,3%
Poland	1,43%	10,46%	13,7%	-	-	-
Portugal	0,43%	7,71%	5,6%	0,55%	5,59%	9,8%
Romania	0,70%	11,91%	5,9%	-	-	-
Russia	1,98%	13,10%	15,1%	-	-	-
Singapore	1,09%	7,04%	15,5%	1,00%	9,73%	10,3%
Sweden	1,31%	7,60%	17,2%	0,53%	6,46%	8,3%
Thailand	1,23%	11,36%	10,8%	-	-	-
Turkey	2,10%	13,82%	15,2%	-	-	-
Taiwan	0,77%	9,47%	8,1%	-0,15%	9,43%	-1,6%
United States	0,90%	8,63%	10,5%	0,42%	9,55%	4,4%
South Africa	1,20%	8,23%	14,5%	1,59%	5,49%	28,9%

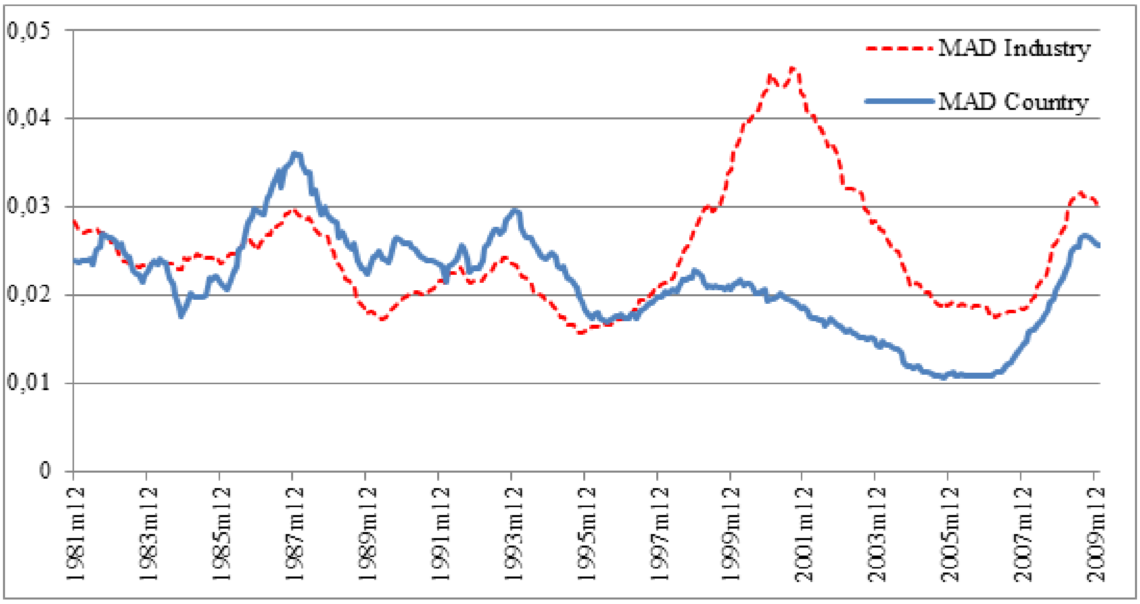
Notes: Mean, standard deviation and Sharpe ratios for value weighted portfolios of country returns for local firms and cross-listed firms. Mean value-weighted returns are obtained by value-averaging firm-month returns for each country for every month, and then averaging for each country for all dates. Value-weighted standard deviations are obtained by calculating standard deviations of firm returns for each country for every month, and then for each country averaging the resulting standard deviations for all dates. Sharpe ratio is the inverse of the coefficient of variation. It is used as a proxy to measure risk adjusted return. Due to limited sample size we do not show results for countries with few cross-listings (see Table I).

Figure 4.1 *Pure Country and Industry Effects (23 month moving average). No cross-listings.*



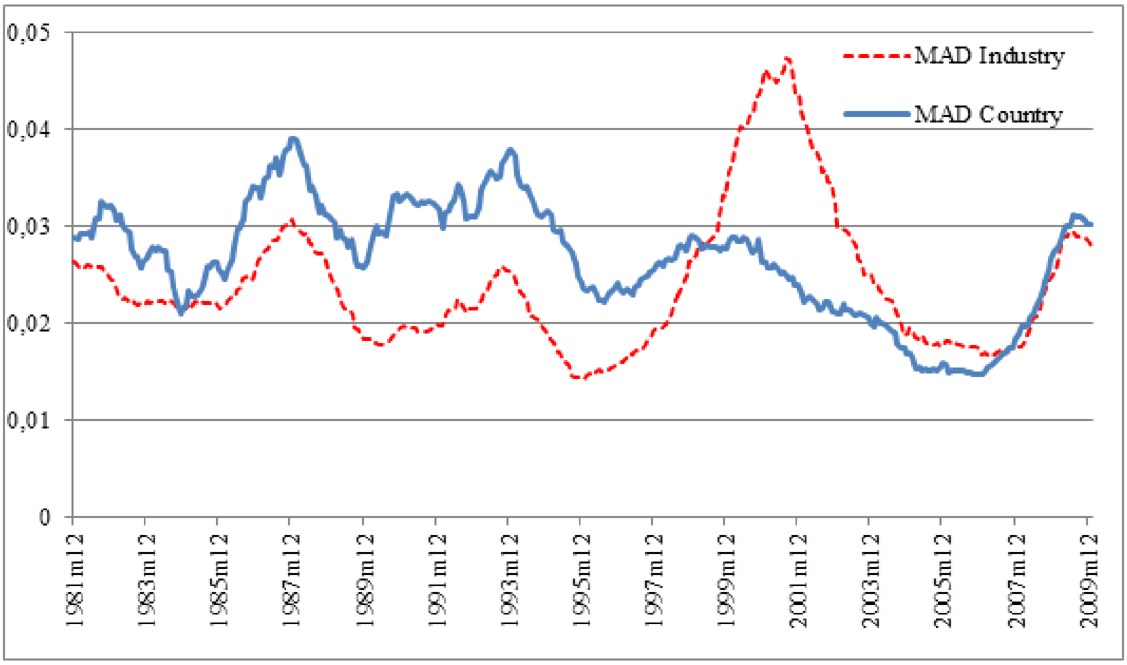
Notes: MADs (mean absolute deviation measures) are calculated following the methodology explained in Section 4. We plot 23-moth moving averages of the monthly weighted absolute values of the industry (red discontinuous line) and country (blue continuous line) effects. Cross-listed firms are not included in this analysis at any stage.

Figure 4.2 *Pure Country and Industry Effects (23 month moving average). Cross-listings are included to calculate the weights in the MADs and in the regressions.*



Notes: MADs (mean absolute deviation measures) are calculated following the methodology explained in Section 4. We plot 23-moth moving averages of the monthly weighted absolute values of the industry (red discontinuous line) and country (blue continuous line) effects. Cross-listed firms are included to calculate the weights of the MADs and in the regressions.

Figure 4.3 *Pure Country and Industry Effects (23 month moving average). Cross-listings are included to calculate the weights in the MADs but not in the regressions.*



Notes: MADs (mean absolute deviation measures) are calculated following the methodology explained in Section 4. We plot 23-moth moving averages of the monthly weighted absolute values of the industry (red discontinuous line) and country (blue continuous line) effects. Cross-listed firms are included to calculate the weights of the MADs but are not included in the regressions.

Figure 4.4 *Correlation between γ_k^D and γ_k^{CL} (23 month moving average).*

Notes: Correlation between γ_k^D (pure country effect attributable to non-cross-listed stocks) and γ_k^{CL} (pure country effect attributable to cross-listed stocks) over time. The construction of γ_k^D and γ_k^{CL} is explained in Section 5.

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